AUTHORS RESPONDING TO COMMENTS

U.S. Fish and Wildlife Service

Mark Gard

PREFACE

This document contains the comments provided by scientific peers on the January 2002 draft of the report, "Flow-habitat relationships for steelhead and fall, late-fall and winter-run chinook salmon in the Sacramento River between Keswick Dam and Battle Creek" (Report), and responses to those comments[1]. This compilation is divided into subject-matter sections whereby various comments and responses to authors were organized. To the extent that individual comments crossed over subject matters, the author collectively addressed those comments.

In addition to the individuals identified on the following page, the Report was also provided for peer review to the following individuals, who did not provide any comments on the Report: Ken Bovee, Midcontinental Ecological Science Center, US Geological Survey; Scott Wilcox, Stillwater Sciences; Tim Hardin; Bill Miller, Miller Ecological Consultants; and Steve Cramer, Cramer and Associates.

Although this compilation may provide useful insight into how the comments were addressed by the author, the Report itself represents the complete and final synthesis of studies on salmonid spawning between Keswick Dam and Battle Creek, based on the best available scientific information. The author has reviewed his responses and compared them to the final Report to ensure that all comments have been adequately addressed. To the extent that any discrepancies remain, the Report itself should be viewed as the final statement.

Lastly, the author of the Report wishes to thank everyone who provided comments on the January 2002 draft. The comments greatly assisted the author and agency in identifying missing or unclear information, focusing the textual and graphic presentations, and thereby producing a better overall Report.

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[1] One of the peer reviewers (Dudley Reiser) reviewed a background document ("Hydraulic modeling of chinook salmon spawning sites in the Sacramento River between Keswick Dam and Battle Creek"), which was provided to the peer reviewers as background material, and did not review the report, "Flow-habitat relationships for steelhead and fall, late-fall and winter-run chinook salmon in the Sacramento River between Keswick Dam and Battle Creek." We have responded to his comments and have incorporated those comments that are relevant to the report, "Flow-habitat relationships for steelhead and fall, late-fall and winter-run chinook salmon in the Sacramento River between Keswick Dam and Battle Creek," into that report.

LIST OF PEER REVIEWERS

Mark Allen
Tom Payne and Associates
Arcata, CA

Gary Smith
California Department of Fish and Game
Sacramento, CA

Ed Cheslak
Pacific Gas & Electric
San Ramon, CA

Dudley Reiser
RZ Resource Consultants
Redmond, WA

Wayne Lifton
Entrix
Walnut Creek, CA

Ed Pert
California Department of Fish and Game
Sacramento, CA

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LIST OF ACRONYMS

ACID	Anderson Cottonwood Irrigation District Dam
CDFG	California Department of Fish and Game
CDWR	California Department of Water Resources
CVPIA	Central Valley Project Improvement Act
HSC	Habitat Suitability Criteria
HSI	Habitat Suitability Index
IFIM	Instream Flow Incremental Methodology
PHABSIM	Physical Habitat Simulation Model
RBDD	Red Bluff Diversion Dam
WSEL	Water Surface Elevation
WUA	Weighted Useable Area

GENERAL COMMENTS

Mark Allen

Comment 1: You have obviously made a significant contribution to the development of spawning HSC in large rivers, but I think the current draft report really lacks the details necessary to convey that information, at least for those who are interested in more than just the bottom line. Following are some comments on your draft, starting with some general organizational comments, then more specific comments with reference to page/paragraph numbers. Please note that I am limiting my comments to the HSC portion of the report.

> **Response:** As noted in our responses to Mark Allen's comments on methods, we have added additional details which will hopefully better convey the information of developing spawning HSC in large rivers.

Comment 2: A couple of significant organizational changes should be made, in my opinion. First, since the WUA results are so intimately related to the HSC, and the HSC effort was a major component of your work, I suggest that the HSC *results* should be described in the "Results" section, rather than in the "Methods". One immediate advantage to that change would be to eliminate all the repetition describing how the HSC were created for each run type (particularly the large, repeated paragraphs describing your adjustment for deep water suitability). Instead of repeating all that for each run-type, the basic data collection and analysis procedures could be described in the Methods section and the run-specific differences (ie the depth ranges of observed redds, and many other items) would be described in the results (along with, of course, the finished HSC curves).

> **Response:** We have implemented part of the suggested changes by describing the basic data collection and analysis procedures in the initial portion of the methods section, but have retained the run-specific differences and finished HSC curves in the portion of the methods section corresponding to each run. We felt that the HSC results better fit in the methods, since the end result of the report is the WUA results.

Comment 3: Another major change is the addition of the actual microhabitat data, which is very lacking and prevents the reader from making any conclusions about the appropriateness of the final curves. For example, you only mention how many redds were observed, the max, min, mean, etc. for those redds, then you show the final fitted curve. Your fitted curves may reflect the actual data very well, or it may be a terrible fit – I have no way of telling without the frequency data from which the curve was derived. There is *no way* that you would let us give such limited data and then jump to the final curves in one of our projects! I really think that most people who are familiar with these studies would want to see the actual data and how the curves fit that data. Also, data describing the habitat availability should also be presented, at the very least the availability data used to estimate deep water suitability should be shown, along with the regression lines for the availability data and the use data.

Other basic data that is lacking and should be presented is the number of observations per segment and habitat type (I believe you only give the number of habitat types and units sampled, but not the number of observations per type. Differences in redd densities according to habitat type could explain some of the differences in your HSC curves, but that data is not presented or discussed.

> **Response:** We have added the actual microhabitat data to the figures of the final fitted curves. We have also added figures showing the availability data used to estimate deep water suitability along with the regression lines for the availability and use data.

Comment 4: Another significant change is the addition of a "Discussion", which could be used to describe/explain differences in HSC according to run-type, comparisons of your HSC with HSC from other locations (both of these would be required by agencies in our HSC reports), differences in WUA results, the relationships between WUA-predicted flows and currently mandated flows, etc. Obviously you are not using this report to state flow recommendations, but still there is a lot of room for discussion of the various results. As it stands now the report just ends abruptly and seems unfinished.

> **Response:** We do not feel that the addition of a discussion section is warranted, given that the purpose of the report is to provide additional scientific information to the FWS CVPIA staff, who are responsible for implementing any revised flow regimes.

Comment 5: Good luck on your revision, Mark. You've got a lot of really good data here. I really do hope you make more of the data visible and expand the discussion. Right now you really require people to blindly trust every decision you made. It seems like a big door for criticism in such a large and important river with listed species. Let me know if you need clarification on any of these comments.

> **Response:** See responses to comments three and four.

<u>**Ed Pert**</u>

Comment 1: Because this is a report that may have no formal formatting requirements, I guess there is no need for a discussion-type section. From my perspective, which may not be appropriate for these reports, I don't know, I am much more comfortable with a summary that includes caveats and uncertainties for the results. As currently written, the report ends with the notion that the methods used in this report are an improvement over the Water Resources report, period. I can't imagine that the differences are that simple. I would like to see a section with a more thorough discussion of areas of uncertainty, etc. to give the reader an idea of how the authors feel about the strengths and weaknesses of the work. I t seems like there was much detail and justification included in the methods. That indicates to me that there should be equal consideration given to the areas of uncertainties of the results somewhere in the document.

Response: We do not feel that the addition of a discussion section is warranted, given that the purpose of the report is to provide additional scientific information to the FWS CVPIA staff, who are responsible for implementing any revised flow regimes.

Wayne Lifton

Comment 1: The work reported presents important and valuable information necessary for making flow-related decisions to support anadromous salmonid spawning in the Sacramento River. The ability to use objective information to support restoration efforts is clearly critical when considering the competing uses for water in California. It represents a significant effort and should be recognized, as such.

Response: We appreciate the compliment.

Comment 2: It appears as if the report is presented in a standard format. I found the report format to be a bit obscure. It would be useful if the Methods section contained a clear statement of objectives and an Approach section. The Approach section should provide a description of the overall methods strategy pertinent to the specific study rather than PHABSIM, in general. It also appears that the Methods section is a combination of Methods and Results, while the Results section is actually a summary.

Response: We feel that the introduction provides a clear statement of objectives and a description of the overall methods strategy pertinent to the specific study. We feel that the overall methods strategy pertinent to the study is identical to the general methods for PHABSIM. We felt that the HSC results better fit in the methods, since the end result of the report is the WUA results.

Comment 3: Another approach to verification was the one that I worked on with Cole Shirvell. That approach is to use your HSC and PHABSIM models to predict whether redds would be present in cells and at flows, where they actually occur. In many respects, this is a more satisfying and more easily conveyed approach. This is particularly useful when dealing with the public and administrators. The results of such comparisons can be evaluated statistically, as well. However, the PHABSIM models to be applied need to be up to the job, which is not always the case. The use of 2D models might be helpful for this purpose.

Response: We used a similar verification method on the Merced River (Gallagher and Gard 1999) and also have been using it on our more recent studies. However, such a verification was outside of the scope of this report, which was to present flow-habitat relationships.

Gary Smith

Comment 1: Review of the draft report generally is limited to those sections detailing development of the habitat suitability criteria (HSC). The review is from the perspective that the report would be published in a technical journal, or as a stand alone publication; technical and editorial comments are included.

> **Response:** We are not intending on publishing this report, as is, in a technical journal. Instead, this report will stand alone to provide scientific information to the FWS CVPIA staff.

Comment 2: The "who, what, where, why, and when" of the study need to be more fully fleshed out. The draft report includes substantial and informative information. However, it is sometimes difficult to follow. It is also unclear why certain steps were taken.

> **Response:** Additional details have been provided as necessary addressing the "who, what, where, why, and when" of the study. The report has been revised to make the information presented easier to follow.

Comment 3: Various "methods" are repeated in the text. I suggest you present like methods once, then describe variations in each run's specific section. This would improve readability.

> **Response:** The suggested change has been made.

Comment 4: The methods should be more fully described. One could not fully repeat the study with the information provided.

> **Response:** Additional information has been added to the methods to hopefully make it possible for the study to be repeated.

Comment 5: The method used to correct depth data and HSC for depth habitat availability raises concern. The method appears to base depth HSC on the availability of suitable water velocities and depths at depth. Relative availability of use and availability are determined and scaled to 1.0. Linear regression (relative use and relatively availability vs specific depth categories) is used to remove "noise" from the data. Linearized use is divided by linearized availability for specifics depths, and again scaled to 1.0. A third linear regression determined the depth at which the scaled ratios reached zero. All the while, it appears that these manipulations use information based on the occurrence of suitable water velocities and substrates at depth, but the link to depth selection to spawning fish does not appear to exist. Further, it is unclear what sort of impact three regressions have on the general character of the original data, and no statistics are provided to demonstrate that linear, rather than curvilinear, regression best describes the original distributions. I suggest you expand this section to address such concerns. I further suggest that you consider another method to address the "depth" issue if such concerns cannot be adequately addressed.

> **Response:** Gard (1998) provides information on the link between the method and depth selection by spawning fish. Figures have been added which show the data and results of the regressions, so that readers can judge whether linear or curvilinear regression best describes the original distributions, and see what impact the regressions had on the general character of the original data.

Comment 6: I suggest you consider revising the report into three components for clarity: 1) HSC data collection and analyses; 2) Physical and hydraulic data collection; and 3) PHABSIM simulations and analyses. All significant points should then be brought together in a conclusions and summary. This is partially done, but more complete partitioning would improve clarity. Each section should have its own introduction, methods, results, discussion, and conclusion.

> **Response:** Physical and hydraulic data collection was addressed in USFWS (1999). We did not feel that it would have added to the clarity to separate the HSC data collection and analysis from the PHABSIM simulations and analysis. We do not feel that the addition of a discussion section is warranted, given that the purpose of the report is to provide additional scientific information to the FWS CVPIA staff, who are responsible for implementing any revised flow regimes.

Comment 7: The Hydraulic Model Construction and Calibration and the Habitat Simulation sections (Pages 21-22) do not include sufficient detail. It should be expanded to provide sufficient information for readers to determine who, what, where, why, and how. Model calibration details should be discussed. I did not make specific comments on the Habitat Simulation Section.

> **Response:** The requested details on hydraulic model construction are given in USFWS (1999). We felt that the Habitat Simulation methods provided sufficient detail for readers to determine who, what, where, why, and how.

INTRODUCTION

Gary Smith

Comment 1: The Introduction should indicate that the study consisted of two significant components, development of site specific HSC and habitat simulation. The importance of using "good" HSC, and the value of developing site specific HSC should be discussed. (Page 1)

> **Response:** We feel that the methods section adequately conveys that development of site specific HSC and habitat simulation were the two significant components of this report. We do not feel that it is necessary to discuss the importance of using "good" HSC and the value of developing site specific HSC in this report.

Comment 2: The Study Area and applicability of the study results should be defined within the Introduction, or within its own section. The title limits the study area, but, as drafted, results could be considered applicable to the "river." (Page 1)

> **Response:** We feel that the last sentence of the introduction makes it clear that the results are not applicable to the Sacramento River below Battle Creek.

Comment 3: Would flows outside of the 3,250-5,500 cfs range be considered if study results indicated they were needed? Or, is that the range, regardless of study results? Additional information would help clarify. (Page 1, Paragraph 1)

> **Response:** The purpose of this study is to provide additional scientific information to the FWS CVPIA program to use in evaluating flow regimes in the Sacramento River. Presumably, these flow regimes could include flows outside of the 3,250-5,500 cfs range. Additional information has not been added since this is outside of the scope of this study.

Comment 4: Mesohabitat and sub-mesohabitat features are important habitat variables as well. You include consideration of mesohabitats later in the report. Perhaps it should be mentioned (and defined) here as well, particularly if you find it important to spawning activities. (You partitioned you sampling by mesohabitats, but did not include information regarding relative importance to spawning fish.) (Page 1, Paragraph 2, Line 7)

> **Response:** We have applied a biologically-based mesohabitat system for spawning where there are two mesohabitat types: spawning habitat and non-spawning habitat. CDFG (1994) found that chinook salmon spawning was not related to mesohabitat type, but instead was directly correlated with gravel permeability. We are using chinook salmon spawning use as a surrogate for permeability. The information on the mesohabitat units sampled for HSC data is given only to show that all possible conditions in the river were sampled for HSC data.

METHODS

Ed Cheslak

Comment 1: A correction for availability was made to the depth suitability curves for a certain subset of the depth range (e.g. 1 to 14 ft for Fall run). This results in several problems. First the HSI is now a combination of utilization and "preference" (i.e., utilization corrected for availability). Second, the velocity HSI curve is based on strict utilization. This causes the HSI curve set to be composed of mixed data types: utilization for velocity and "preference" for depth. I suggest that you do not correct the depth curve for availability.

> **Response:** The commenter is correct that the criteria has part of the depth corrected for availability but with velocity uncorrected. We view this as appropriate since this method was published in a peer-reviewed journal (Gard 1998). We do not agree that the depth curve should not be corrected for availability, since most of the decline in habitat use with increasing depth (in fact all of the decline for steelhead) can be attributed to the availability of habitat with suitable velocities and substrates. Failure to correct the depth curve would result in criteria which are biased towards shallow depths.

Comment 2: Suitable spawning gravels are deposited at specific locations in the river because of the sediment transport process active in the river. When it comes to spawning locations (i.e., locations in the river where suitable gravels exist for spawning), depth and velocity are covariates that are both influenced by fluvial processes. That is, these processes both affect and are effected by the depth and velocity distributions in the channel. Consequently, it becomes extremely difficult to tease the independent influence of depth from the correlated influence of velocity on suitability. My suggestion is to use the lower end of the depth utilization curve to identify where depth is not sufficient to support spawning and when utilization tails off (at greater depths) set the suitability to 1.0 so that WUA is driven by only two variables (velocity and substrate) - i.e., depth is no longer influencing the suitability of a given spawning location.

> **Response:** The method used to correct depth for availability (Gard 1998) separates the effects of depth versus velocity and substrate by using the same ranges of velocity and substrate for each depth interval. Setting the suitability to 1.0 for depths greater than the peak of the curve would (for chinook salmon) result in criteria which are biased towards large depths. For chinook salmon, the depth correction analysis indicated that the ratio of use to availability declined with increasing depth, indicating that depth does influence the suitability of a given spawning location. In contrast, we did apply the commenter's suggestion for steelhead, since for steelhead, the ratio of use to availability increased with depth, indicating that use was solely controlled by availability.

Comment 3: I did not understand the weighting you used to obtain segment WUA. Maybe more explanation would help. Typically transects are placed in certain mesohabitats that occur with a particular relative abundance in the segment. The weighting comes from this relative proportion of mesohabitat as one extrapolates to an entire segment. Instead the ratio of total redds in a segment to the redds in a modeling site was used. I did not understand how this adjustment gave rise to an appropriate estimate of the surface area of river in a given segment that was suitable for spawning.

> **Response:** We have applied a biologically-based mesohabitat mapping system for spawning where there are two mesohabitat types: spawning habitat and non-spawning habitat. CDFG (1994) found that chinook salmon spawning was not related to mesohabitat type, but instead was directly correlated with gravel permeability. We are using chinook salmon spawning use as a surrogate for permeability. All of the transects were placed in the spawning mesohabitat type, since there is no spawning habitat associated with the non-spawning mesohabitat type. Thus, the proportion of spawning habitat in the segment which is found in the spawning mesohabitat type is 100%. Since we used a biologically-based mesohabitat mapping system, we also used a biologically-based weighting system - the number of redds. Thus we extrapolated to the entire segment by using the proportion of spawning in the segment which was found in the spawning sites. The assumption of this method is that the proportion of spawning habitat in a segment which is found in the spawning sites is the same as the proportion of redds in a given segment which were found in the spawning sites. This assumption is supported by Gallagher and Gard (1999), who found that the number of redds in a spawning site was correlated to the amount of spawning habitat in that site. Thus, this extrapolation technique gives an appropriate estimate of the amount of spawning habitat in a given segment.

Comment 4: A map would be very useful here to show the segments. Also a table giving the values for the six segments used to distinguish them would also be useful. e g., hydrology, gradient, area of spawning, etc. (Page 2)

> **Response:** A map has been added (Figure 1) showing the segments. The figure shows the average flows for the period October 1974 to September 1993 at the top of each segment. These flows were the main parameters used to distinguish the segments. Bovee (1995) recommends that the cumulative change in flow within a segment be less than 10%. Segment 6 was separated from Segment 5 because Segment 6 is not typical of the rest of the Sacramento River (CDWR 1993), with regards to channel morphometry, and also because of the significant differences in the velocities and depths above ACID when boards are in versus out at ACID. Segments 5 and 4 were separated because the cumulative flow increases by more than 10% from below ACID to below Cow Creek. Lake Red Bluff (the 6.5 miles above RBDD) was separated from Segment 4 because it is inundated four months of the year when the gates are in at RBDD. Segments 4 and 3 were separated because Lake Red Bluff is in between the two segments. The downstream end of Segment 2 was selected because the cumulative flow increases by more than 10% from below RBDD to below Deer Creek.

Comment 5: It would be nice to know the total number of spawners these percentages are based on. (Page 2)

Response: The CDFG redd counts are only accurate as an index (to estimate spatial distribution and relative numbers from year to year), and not as to actual numbers of spawners. Thus, while the percentages are probably relatively accurate, the numbers of redds that these percentages are based on is probably not accurate.

Comment 6: Neither this description nor the 1999 paper provided sufficient detail on site selection for me to reproduce the process. I suggest more detail here especially for readers who want to select sites for similar studies. (Page 2)

Response: The 1999 paper (USFWS 1999) states "the mesohabitat units were ranked in each of the stream segments, to identify those areas which consistently received the highest spawning use." For each year and run, the mesohabitat with the highest number of redds received a rank of 1. In the event of ties, all of the mesohabitat units with the highest number of redds received a rank of 1. The mesohabitat unit with the next highest number of redds received a rank equal to one plus the number of mesohabitat units with a rank of 1 (if there were three mesohabitat units which tied for the most number of redds, then the mesohabitat unit with the next highest number - the fourth mesohabitat unit in the list of ranked units - would have a rank of 4.) The ranking proceeded in this fashion, with all ties receiving the same rank, until the mesohabitat unit(s) with the lowest number of redds had been ranked. The mesohabitat units were ranked for all six years of redd count data for each run in this manner, and the total rank for each mesohabitat unit was the sum of the (six) ranks for the six years. The top ranked mesohabitat unit for a given run was the unit which had the lowest number for the total rank For Segments 5 and 4, where we were modeling all three races (fall-run, late-fall-run and winter-run), we selected units which were the top-five ranked for at least one race, while for Segment 6, where we were primarily modeling for late-fall-run, we selected the top-four ranked for late-fall-run. The above mesohabitat units are listed in Table 1 in USFWS (1999). The process used to select the sites which were actually modeled from the sites in Table 1 of USFWS (1999) is presented in USFWS (1999).

Comment 7: Why were there different numbers of transects at each site? (Page 2)

Response: The length of each site was selected to cover the area which is used for spawning. Each transect represented a distance above and below each transect. The number of transects at a given site depended on the complexity of the site (ie how much of a distance above and below each transect was represented by the transect) and the length of the site, with more complex and longer sites having more transects than shorter and less complex sites.

Response-to-Comments Document

for the

January 2002 Peer-Review Draft of the
Sacramento River Spawning Flow Study Report

February 2003

Comment 8: Transect locations are critical in 1-D studies (as you know) so re-iterating the criteria in USFWS (1999) is appropriate. Have redd surveys since 94 verified these selections? (Page 2)

> **Response:** The criteria from USFWS (1999) (transects were located to cross the areas most heavily used by spawning chinook salmon) has been added to this report. We have not examined redd survey data collected since 1994 to see if later CDFG aerial redd survey data is consistent with the data used to select our sites. However, we did find substantial spawning in our study sites during HSI data collection in 1995 to 2001.

Comment 9: How were discharges chosen? Did they span the range of natural spawning flows or management options? (Page 3)

> **Response:** The discharges were not specifically chosen, but were the discharges which were available for measurement during the data collection efforts. We were able to extrapolate down from the lowest discharge at which WSELs were collected (4075 to 4950 cfs) to the lowest permitted release from Keswick Dam (3250 cfs). The highest discharge at which WSELs were collected (25,000 to 35,000 cfs) falls within the range where flows are generally uncontrolled due to releases for flood control. Except during flood-control releases, the highest typical release from Keswick Dam is 15,000 cfs. Thus, the discharges at which WSELs were collected allowed us to simulate habitat in the range of flows available for management options.

Comment 10: This implies that only one velocity data set was obtained. Is that true, and if so how did you calibrate the hydraulic model? (Page 3)

> **Response:** The reviewer is correct that (for the most part) only one velocity set was obtained. The current standard practice in instream flow studies is to collect one velocity set, with only WSELs being calibrated (Bovee 1995). We are not aware of any standard practice for calibrating velocities for PHABSIM studies.

Comment 11: You should state that the best estimates of flow curve have gage data and that transect measured Q was usually $\geq$ 5% gage Q. Great results! (Page 3)

> **Response:** This information is given in USFWS (1999) and readers are directed to that report for details on hydraulic and structural data collection, including the above information.

Comment 12: With info given in USFWS (1999) I cannot independently verify these measurement Q accuracies. (Page 3)

Response: The measurement Q accuracies can be verified by comparing the data in Tables 4 and 5 in USFWS (1999). For example, for the Salt Creek site, the percent difference (3%) would be calculated as: (14600 cfs - 14228 cfs)/14600 cfs. The gage flow for each velocity set is also given in Appendix C.

Comment 13: I'm sure the site [where depths and velocities were measured] had similar gravels/substrate to the pit - but that should be mentioned. (Page 4)

Response: The gravel/substrate in the pit is typically larger than the substrate at the substrate measurement locations (in front of the pit, on the sides of the pit and in the tailspill), since larger particles which the salmon were unable to move are left in the pit. The substrate in the Sacramento River tends to be armored, so that the material in the pit would be larger than the material which was on the surface at the pit location before the redd was constructed. We concluded that the substrate sizes at the measurement locations were the best way to estimate what the substrate size was on the surface at the pit location before the redd was constructed.

Comment 14: Were your velocity meters calibrated in any way? If so how? (Page 4)

Response: We did not calibrate our velocity meters. Price AA meters are assumed to be measuring the velocities correctly if they pass a spin test. We regularly perform spin tests on our Price AA meters. Marsh-McBirney meters are not generally calibrated. We did check the velocity measurements on our Price AA and Marsh-McBirney meters on May 17, 2001 using the rotating rating tank at the UC Davis Hydraulics Lab. The tests were performed with a depth of 1.0 feet. The tests indicated that our velocity meters were sufficiently accurate. The results of the tests were as follows:

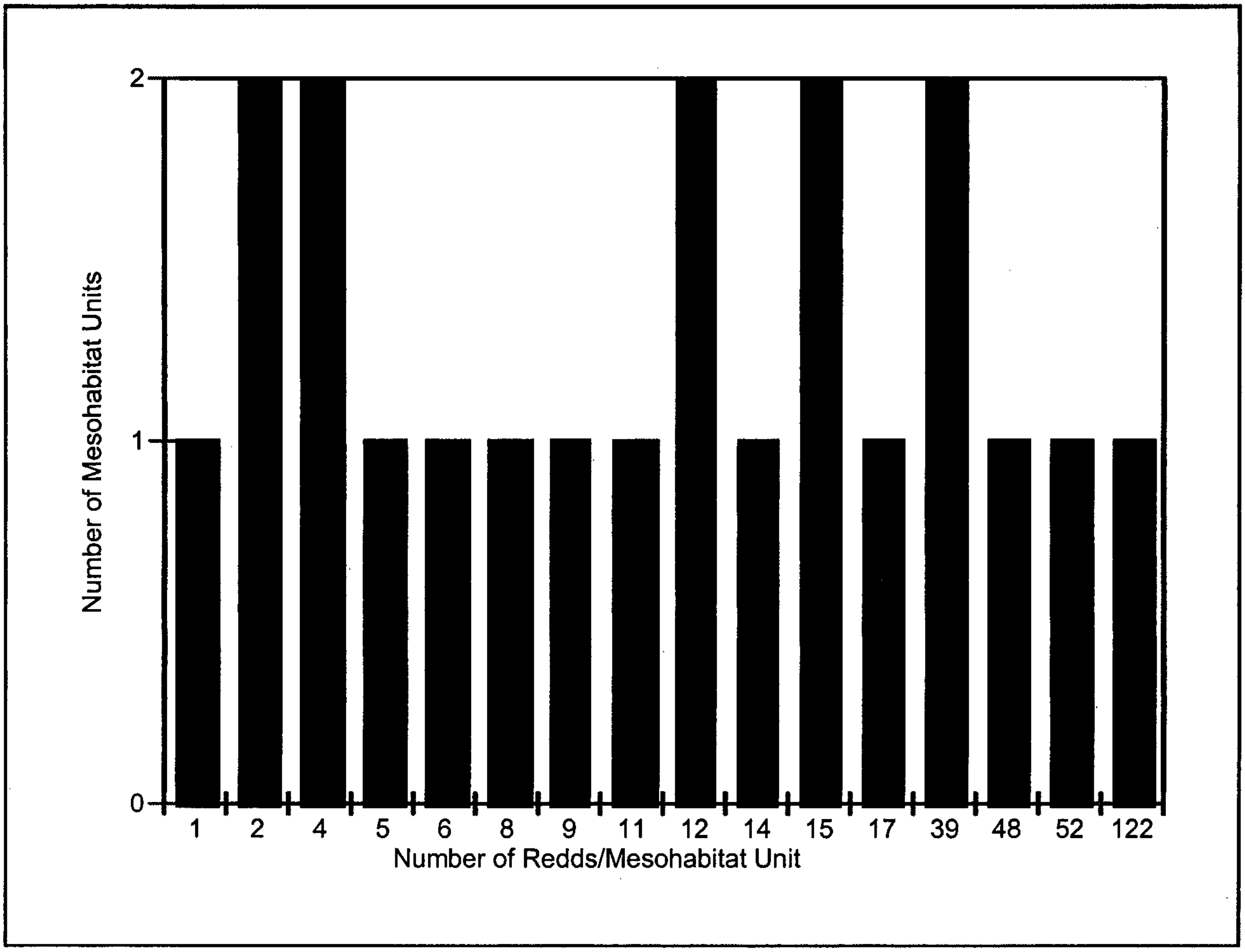
Knownvelocity(ft/s)	Price AA velocities (ft/s)	Marsh-McBirney velocities (ft/s)
1.23	1.28-1.31	1.22
1.99	2.10-2.12	1.94
3.16	3.22-3.31	3.22
0.27	0.27-0.28	0.29

Comment 15: It would be interesting to note the run sizes in these years. Also, any evidence of superimposition? (Page 5)

Response: CDFG carcass surveys indicated that the run size for fall-run chinook salmon between Keswick Dam and Red Bluff Diversion Dam was 26,548 in 1995, 28,890 in 1996, 26,191 in 1997 and 18,295 in 1999 (CDFG 2000). Potential superimposition of fall-run chinook salmon redds in 1995 was noted by CDFG (1997).

Comment 16: The distribution of your sampled redds (ie #/unit) for each mesohabitat would be useful. Also some indication of sampling effort in these units would also help. (Page 5)

Response: The distribution of the fall-run redds in the mesohabitat units sampled is shown below. With regards to sampling effort, we sampled all of the area of each mesohabitat unit that we sampled.



Comment 17: Elimination of points is controversial and a greater treatment of that process is needed so a reader can follow your reasoning and appraise the consequences. (Page 5)

> **Response:** The original curves were printed out and a ruler was used to draw lines onto the curve that came close to overlaying the original curve. The points at the intersection of the lines were kept and the remaining points were deleted. The simplified curve was printed out and overlain with the original curve to ensure that the simplified curve came close to overlaying the original curve. We have added bar graphs showing the original data to the final criteria curves, so that the overall effect of curve fitting and elimination of points can be seen graphically.

Comment 18: I suggest including these results in an appendix - there is no way to independently evaluate the "qualities" of the regressions, etc. (Page 5)

> **Response:** We disagree. We feel that these details are important to allow the reader to understand the process that was used to derive the criteria. However, we have added figures showing the data points and results of the regressions to allow readers to independently evaluate the "qualities" of the regressions for the depth correction method.

Comment 19: Many readers may not be familiar with CURVE. I suggest showing the frequency distribution with the curve results superimposed as a figure. Then readers could judge for themselves how effective CURVE was in developing a smooth fit to the data. (Page 5)

> **Response:** We have changed the figures of the depth and velocity criteria to show the frequency distribution superimposed on the curve results.

Comment 20: In results you discarded DWR study because it corrected utilization to obtain preference. Aren't you doing the save thing with depth. Why correct? Now you have a heterogeneous HSI set with part of depth corrected for availability and velocity uncorrected. This seems very irregular. Finally, your correction method is novel but not related (easily) to the electivity indices normally used to correct for availability. Need to build a theoretical bridge to justify correction. (Page 6)

> **Response:** We are not using the same technique as the DWR study (which used preference ratios) in correcting depth suitability for availability. We corrected the depth suitability because most of the decrease in use associated with increasing depths can be attributed to availability of habitat with suitable velocities and substrates. The commenter is correct that the criteria has part of the depth corrected for availability but with velocity uncorrected. We view this as appropriate since this method was published in a peer-reviewed journal (Gard 1998). Gard (1998) provides the theoretical bridge between electivity indices (preference ratios) and the method used in this report.

Comment 21: Supposition; these variables are correlated and cannot easily be separated. I do not believe a "correction" is justified, despite the reference. (Page 6)

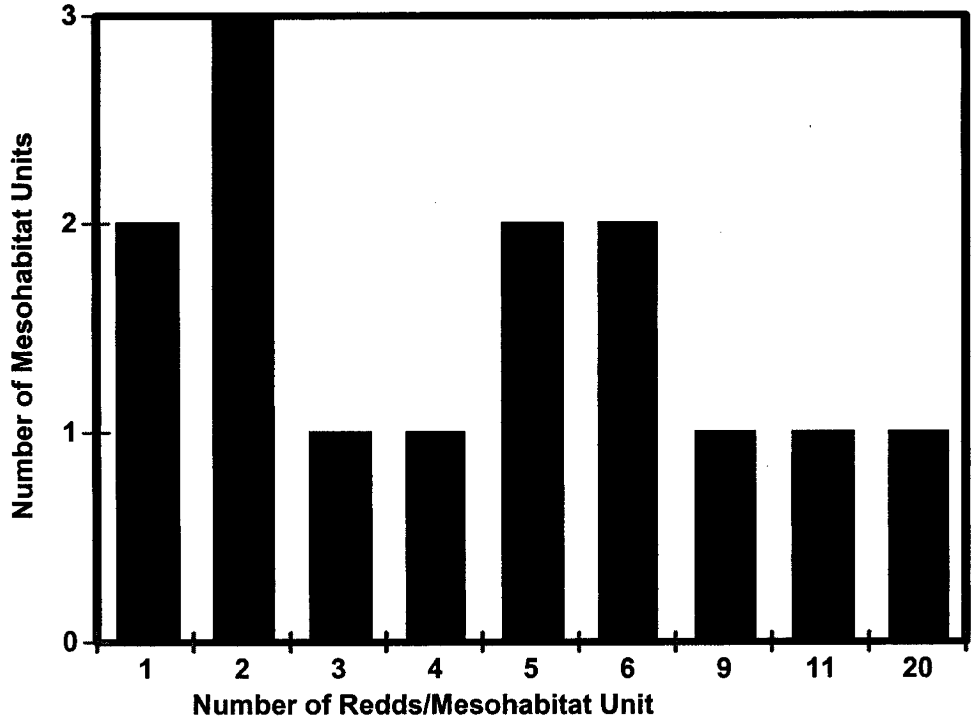
Response: This supposition (that decreasing suitability is likely due to low availability of deeper water with suitable velocities and substrates) follows directly from the results of the method. The method separates the effects of depth versus velocity and substrate by using the same ranges of velocity and substrate for each depth interval. We disagree that a correction is not justified since the method being applied has been published in a peer-reviewed journal (Gard 1998).

Comment 22: Please provide citation for this criterion. (Page 9)

Response: The criterion of a minimum of 150 observations of redds is on Page 54 in Bovee (1986).

Comment 23: Again, the distribution of redds across the mesohabitats would be useful. (Page 9)

Response: The distribution of the late-fall-run redds in the mesohabitat units sampled is shown below.



Comment 24: A more powerful test would be the χ^2 goodness of fit test, using your fall run as the "real" distribution. (Page 9)

Response: The proposed test would not be appropriate since it would not take availability into consideration, unlike Thomas and Bovee's (1993) transferability test. In this regard, Thomas and Bovee (1993) state: "The fact that a destination stream may have very little optimum microhabitat is poor justification to redefine what is optimum for the species." The power of a test is the ability to detect the alternative hypothesis when the alternative hypothesis is true, or not making a Type II error (Steel and Torrie 1980). For a transferability test, a Type II error would be rejecting transferable criteria (Thomas and Bovee 1993), as was done in this case. Thomas and Bovee note that "rejection of transferable criteria is inconvenient but not as serious as using the wrong criteria in a study." Further, Thomas and Bovee (1993) found that Type II errors were negligible when at least 55 occupied and 200 unoccupied locations were used in the test. Since we used 77 occupied locations and 2,219 unoccupied locations, the chances of a Type II error in this case were negligible, and thus the transferability test was sufficiently powerful.

Comment 25: Simulations do not constitute empirical data for use in such comparison. Simulations are repeated observations at the same location (ie transect) made without error. This violates sampling assumptions for use of χ^2 analysis. I know Bovee uses this approach but it has been seriously criticized. (Page 10)

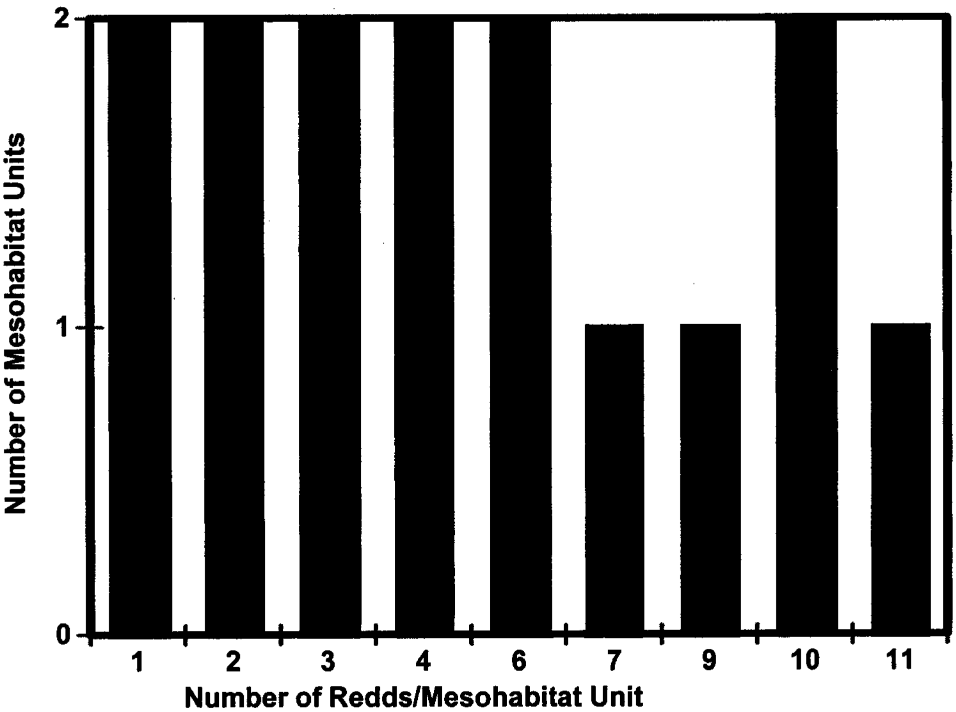
Response: We only used one observation at the same location (ie vertical) for our unoccupied data set. Observations at different verticals are at different locations, even if the verticals are on the same transect. Errors in velocity measurements at each vertical are carried forth to the simulated velocities at each vertical, and thus the simulated velocities do have an error associated with them. Thus, we did not violate the sampling assumptions for use of χ^2 analysis.

Comment 26: This also grossly over-estimates the unoccupied cells - it amounts to multiple counts of the same location. (Page 10)

Response: See response to comment 25. Since we only used one observation at each vertical, we did not have multiple counts of the same location.

Comment 27: Specify the mesohabitats where they collected these and how they fit with your data set. (Page 11)

Response: It is unknown what mesohabitats the CDFG data (from 1986 to 1988) were collected in, since mesohabitat typing was not performed until 1995. From the rivermile designation for each redd, redds were found in two mesohabitats below Battle Creek; the Sacramento River has not been mesohabitat-mapped below Battle Creek. Of the remaining redds, based on the rivermile designation, it appears that, for CDFG's data that we used, the late-fall-run redds were in two Bar Complex (BC) Glides, two BC Riffles, one BC Run, two Flatwater (FW) riffles, three FW Runs, two FW Pools and one FW Glide. The distribution of CDFG's late-fall-run redds which we used in the mesohabitat units sampled is shown below. Since we are using a biologically-based habitat-mapping system for spawning, the distribution of CDFG's data across mesohabitat types has no effect on how CDFG's data fits with our data.



Comment 28: Since this is the same as before just say that instead of repeating text. (Page 11)

Response: We have changed the text to "Substrate criteria for late-fall-run were developed using the same methods as for fall-run."

Comment 29: Eliminating points after exponential smoothing is problematic. You need to provide solid, independent justification for these deletions. (Page 11)

Response: See response to comment 17. The bar graphs added to the criteria figures provide independent justification for the combined effects of curve fitting and elimination of points, showing that the final criteria provide a good fit to the original use data.

Comment 30: There are important hydrodynamic reasons why salmon may be selecting shallow depths. I do not agree with your argument here. Need to supply independent observations/data showing that deeper depths are utilized. (Page 11)

Response: We agree that there are important hydrodynamic reasons why salmon may be selecting shallow depths. Specifically, relationships between intragravel velocity and water column velocity and depth indicate that use would decrease faster than availability with increasing depth to ensure sufficient intragravel velocities (Gard 1998). The purpose of the method in Gard (1998) is to separate such effects from the effects of availability. We measured late-fall-run redds in depths up to 9.7 feet, while we measured fall-run and winter-run redds in depths up to, respectively, 13.8 and 15.6 feet. Thus, deeper depths are utilized by spawning chinook salmon. See also our response to comment 21.

Comment 31: Don't repeat text; just refer readers to previous treatment - makes easier reading I think. (Page 12)

Response: We have modified this text so that only those aspects that changed from run to run, (such as suitable velocities between 0.9 and 2.82 ft/s) are given in the text for each run, with a general description of the depth correction process given in the initial portions of the methods.

Comment 32: Fluctuating flow conditions may be typical during the spawning season of all races of chinook. Thru, holding flows constant, or only measuring during constant flow, may be atypical or cause skewed results. There seems to be a "range" of velocities/depths which are suitable and we could be fundamentally mis-representing this by only measuring at a steady flow. Plus, it is not just non-fluctuating flows, but flows similar to when spawning occurred. (Page 12)

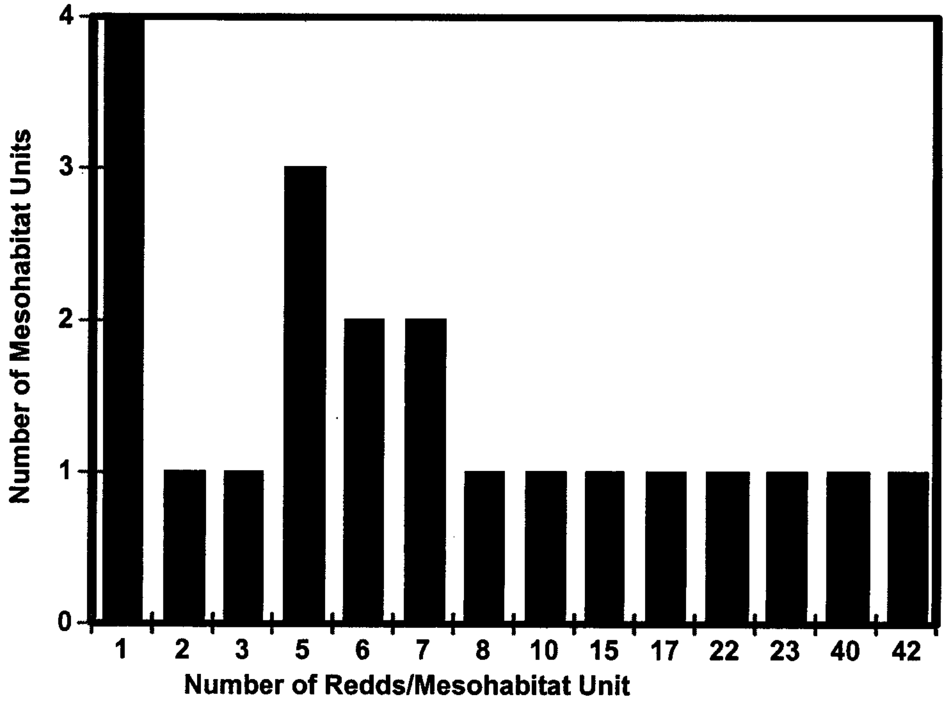
Response: The reason to have non-fluctuating flows is to have the flows during sampling be similar to those when spawning occurred. Thus, in this context "non-fluctuating flows" is synonymous with "flow during sampling similar to those when spawning occurred." We feel that the decrease in uncertainty in our measurements of depths and velocities associated with constant flows is more important that potential atypical or skewed results that could come from holding flows constant or only measuring during constant flows. We feel that we have avoided problems associated with constant flows by gathering data in multiple years, where the flows were different each year. Further, the Sacramento River has enough diversity of habitat conditions that even at a constant flow, fish are able to select a wide range of velocities and depths, as reflected in the criteria.

Comment 33: Do you really believe that Fall-run have 0 HSI at 48ft and late fall at 18.9 ft? I'm not sure I do. (Page 12)

Response: Gard (1998) states: "The depth at which the modified HSC curve reaches zero has no biological significance; rather, it is a convenient way to describe in the criteria the rate of decline in suitability with depth." Since the fall-run and late-fall-run data were collected at relatively similar flows, and thus similar availabilities, the slower rate of decline in fall-run depth suitability (versus late-fall-run) reflects that the use data for fall-run dropped off slower than the use data for late-fall-run. For example, the deepest fall-run redd had a depth of 13.8 feet, while the deepest late-fall-run redd had a depth of 9.7 feet.

Comment 34: sample distribution. By mesohabitat type? (Page 14)

Response: The distribution of the winter-run redds in the mesohabitat units sampled is shown below.



Comment 35: Same text - redundant, so just cite previous methods. (Page 16)

Response: We have changed the text to "Substrate criteria for winter-run were developed using the same methods as for fall-run."

Comment 36: I am beginning to believe that these species are not responding to depth as a criterion. (Page 17)

Response: We disagree. The criteria indicate that the three races of chinook salmon do respond to depth as a criterion.

Comment 37: Now lets consider what these curves are saying to us (biologically). Are we going to believe the steelhead curves (from American River) that show no diminishing suitability with depth for a species that is smaller in size than chinook and invoke curves for chinook that diminish linearly with depth? Something seems wrong to me. Chinook should, in my experience be less sensitive to depth (at values > 5') than steelhead. These chinook curves do not agree with my "intuitive" perceptions. (Page 19)

Response: The analyses for chinook salmon indicated that ratio of use to availability decreased with increasing depth, thus resulting in a decline in suitability with depth. In contrast, the analysis for steelhead (USFWS 2000) showed that the ratio of use to availability increased with increasing depth, indicating that use was entirely controlled by availability. In this case, Gard's (1998) method results in the depth HSC to be modified to have a suitability of 1.0 for all depths greater than the original peak of the curve. We are not aware of any biological reason why larger fish would necessarily have a greater preference for deeper waters than smaller fish. It is likely that the different results for chinook salmon and steelhead reflect differences in the hydraulic characteristics in the areas in which they spawn. These hydraulic differences could just as likely cause smaller fish to have greater suitability for deeper waters (versus larger fish), versus causing smaller fish to have a lesser suitability for deeper waters. It is possible that the results of the analysis differ from the commenter's "intuitive" perceptions because the commenter's "intuitive" perceptions are wrong.

Comment 38: Shouldn't this be WUA for each transect. Transects x weight = segment. (Page 22)

Response: Weighted useable area for transects can be presented either with units of square feet per 1000 feet, or with units of square feet. In this case, we are using units of square feet for WUA for transects, in which case the WUA for the transects are summed to produce the WUA for the segment.

<u>Mark Allen</u>

Comment 1: Shouldn't there be a figure showing the segments? People who don't live or work on the river would want to "see" where they are. (Page 2)

Response: Figure 1 has been added showing the segments.

Comment 2: The percentages are for 1989 to 1994, doesn't CDFG have more recent data available (ie, during the years of your study). Did these percentages change after the Jan '97 flooding? Where are the percentages for STH? Maybe this table would be an appropriate place to list the number of study sites sampled by segment for each run-type. (Page 2, Table 1)

> **Response:** The data in Table 1 is the data which was used in 1995 to select which reaches should be modeled for each run. CDFG has continued to conduct aerial redd surveys. For our 1999 annual report, we looked at CDFG data for fall-run for 1989-98. The distribution of fall-run redds for that period was 7% in Segment 6, 36% in Segment 5, 9% in Segment 4, 27% in Segment 3 and 15% in Segment 2. CDFG does not have aerial redd count data for steelhead. Spawning habitat was modeled for all runs in all of the study sites in Segments 4, 5 and 6; accordingly, it is not necessary to add this information to Table 1.

Comment 3: Here you use the term "HSC", whereas in many other places you use "HSI". Maybe there actually is a difference in the definitions of the two, but I don't know it and I found it very confusing when you alternate between the two terms. (Page 4, Title)

> **Response:** Habitat Suitability Criteria (HSC) refer to the overall functional relationships that are used to convert depth, velocity and suitability into habitat quality. Habitat Suitability Index (HSI) refers to the independent variable in the HSC relationships. The report has been modified to use HSI only in situations (HSI value and HSI curve) where it refers to the independent variable in the HSC relationships. Hopefully this change has made the report less confusing in this regard.

Comment 4: Both present and past tenses are used here and in many areas throughout the report.(Page 4, Par 2)

> **Response:** We have used the past tense in referring to events taking place in the past (for example data collection efforts) and have used the present tense in suituations such as describing equipment which is still existing. We felt it would suggest that the equipment no longer existed if we referred to it in the past tense.

Comment 5: Was CDFG redd distribution data used to select sampling areas? Were the CDFG surveys based on aerial photos and biased by shallow water spawning? If so, could that effect the relative number of shallow versus deep redds found during your surveys (ie if aerial surveys showed most spawning in shallow riffles, when most spawning actually occurred in deeper runs, if mostly the shallow riffles were surveyed the suitability of deep water might not be accurately estimated). (Page 4, Par 2, Sent 6)

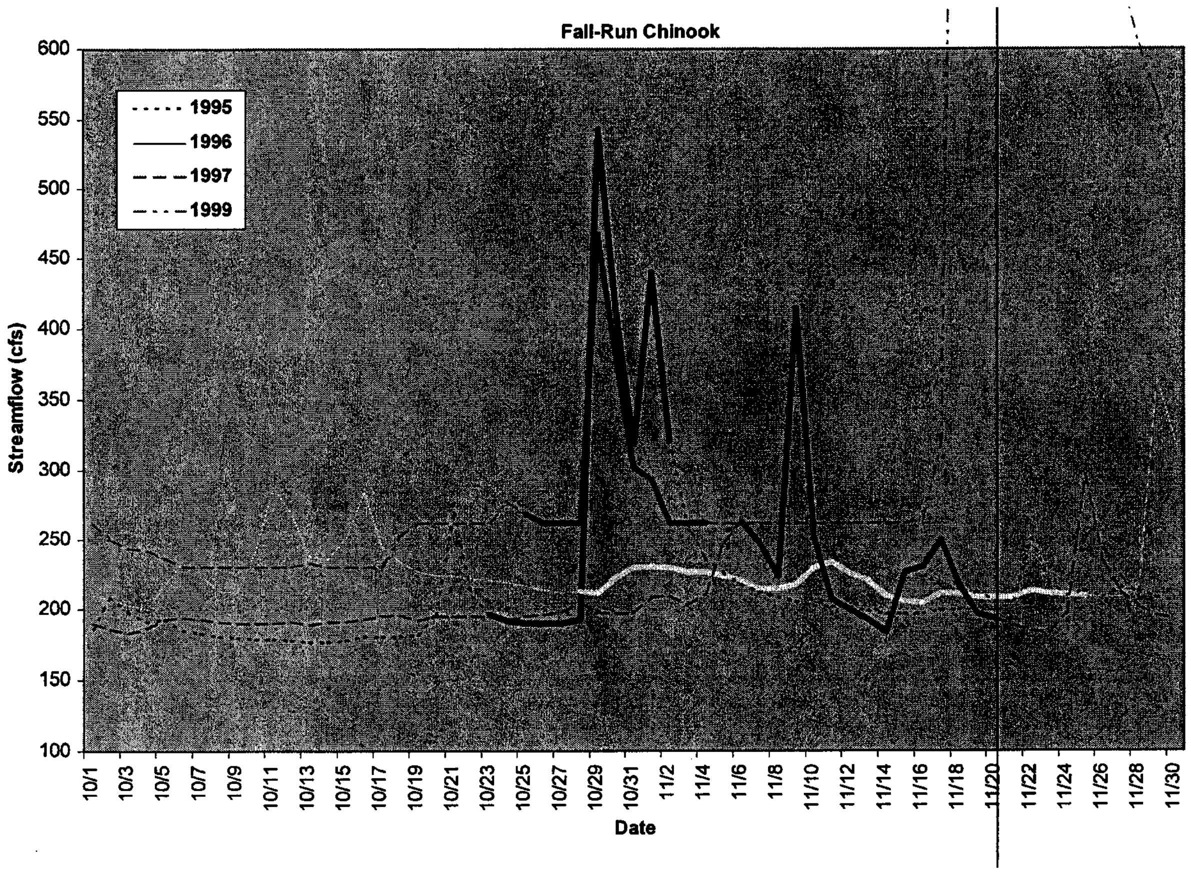
Response: We didn't exclusively sample areas where CDFG had seen redds in their aerial redd surveys. The CDFG surveys were based on observations during the flights; these observations could well have been biased towards shallow areas, particularly during periods of lower water visibility. We avoided such a bias in our sampling by spending equal effort sampling deeper areas (greater than three feet) versus shallow areas. For fall-run salmon, we sampled a total of ten riffles and ten runs. For late-fall-run salmon, we sampled a total of eleven riffles and nine runs. For winter-run salmon, we sampled a total of 26 riffles and twelve runs. If sampling more riffles had biased the winter-run sampling to shallower depths, you would expect that the winter-run criteria would have shown shallower depths having a higher suitability; in fact, the winter-run criteria show a lower suitability for shallower depths than do fall-run and late-fall-run criteria.

Comment 6: This sentence was confusing and should be rewritten. I'm guessing you mean that the parallel runs were made in an upstream direction. I also assume that the parallel runs were made adjacent to each other so that significant areas were not skipped. If so, how was the boat position determined to ensure adjacent transects? (Page 4, Par 2, Sent 7)

Response: This sentence has been rewritten to indicate that parallel runs were made in an upstream direction. The parallel runs were made adjacent to each other so that significant areas were not skipped. The boat position was determined by starting at the same location (upstream versus downstream), with the boat driver noting by eye his starting position across the channel relative to his starting position on the previous run. Runs were made with the first run near one bank and finishing with the last run near the other bank.

Comment 7: You should present the flow data in a figure as you did for the other run types, however I would suggest a different format that would show not only the flows that occurred during sampling each year, but would also show the typical range of flows during the full extent of the spawning season. For example, a figure would show the flows over the full course of the spawning season for a particular run for each year that data was collected (eg, for 1995, 1996, 1997, and 1999 for fall-run). A thicker line style could be overlaid on each line to denote the period of actual sampling for HSC data. That way the reader could judge for themselves if sampling occurred during flows typical of the flows that occur I'm thinking of during the spawning season, and they could visually see the differences in flows between years. After all, a picture is worth a thousand words. See the example [below] as an illustration of the figure(this uses flows from another location): (Page 5, Par 1, Sent 2)

Response: Flow data has been added for fall-run. We have modified the flow data figures for all of the runs in a similar manner to what is suggested by the commenter. However, we had to keep each year separate, since with black and white, it was difficult to distinguish one year from another. We also added a vertical line to indicate the end of the sampling period.



Comment 8: If you did collect some data in 1998 could it be used to compare depths and vels with the other data as a sort of qualitative validation? (Page 5, Par 1, last Sent)

> **Response:** Measurements were only made on two fall-run redds in 1998, which would not make it possible to do the qualitative validation suggested.

Comment 9: In this and other run-types you mention your equalization of "sampling days" according to depth strata. Comparative surface areas would be a much better way of assessing the relative levels of effort, especially since it probably takes much longer to survey a given area of deep water using the boat and video equipment than it would to survey shallow water by wading or diving. Consequently, expending equal numbers of days might not constitute "equal-area sampling", if that was your goal. However I will acknowledge that most people don't even try to account for differences in effort in margin versus midchannel areas, so I appreciate your efforts. (Page 5, Par 2, Sent 2)

Response: We actually found that we were able to cover approximately the same area of deeper habitat in the same time with the boat and video system versus shallower habitat by wading.

Comment 10: As I mentioned before, this data should be presented in more detail in a table (ie how many units of each type in each segment), and why not also show the locations on the map that you will add (right?). (Page 5, Par 2, Sent 3)

Response: We disagree. We do not feel that the number of mesohabitat units sampled in each segment, or the location of the mesohabitat units on Figure 1 is relevant, since criteria were derived for the entire river. We feel that the text as is gives enough information about the sampling effort among different mesohabitat types, particularly since we are only using one biologically-based mesohabitat type (spawning habitat) for spawning (See response to Ed Cheslak's Comment 3).

Comment 11: This sentence needs to be reworded (and the same sentence for each of the other run-types also, unless of course it all goes into a single Methods description, which would again eliminate the need to repeat it over and over). Specifically, you mention truncating the "lower" end and taking suitability to zero "below" the "lowest" observed value. It should read "The curves generated . . . truncating at the fastest/deepest end, so that the next deeper or faster value had an index of zero", or something like that. Also, your description of "eliminating points not needed to capture the basic shape of the curves" is very vague and should be described more fully in order to avoid arousing suspicion among those who may not know your quantitative abilities. I understand that you are likely talking about eliminating small dips and humps in faster or deeper water, but many people would not know what kind or what magnitude of subjective changes you are making. For that reason, I typically try to show my raw fitted curves along with my modifications so others can judge for themselves if I am taking liberties or trying to bias the curve. You don't provide any information on your modifications or even a simple description of what you do. (Page 5, Par 3, Sent 3)

Response: The above sentence has been reworded similar to the manner suggested. This sentence actually referred to the slowest/shallowest end, rather than the fastest/deepest end. The procedure used to eliminate points from the curves was as follows: 1) the original curves were printed out and a ruler was used to draw lines onto the curve that came close to overlaying the original curve; 2) the points at the intersection of the lines were kept and the remaining points were deleted; 3) the simplified curve was printed out and overlain with the original curve to ensure that the simplified curve came close to overlaying the original curve. We have added bar graphs showing the original data to the final criteria curves, so that the overall effect of curve fitting and elimination of points can be seen graphically. We feel that this should address the above concerns about the elimination of points without having to add the above text to the report. The corresponding sections for late-fall-run and winter-run have been replaced with a reference back to the fall-run methods.

Comment 12: This entire paragraph was very confusing, and if the HSC were moved into the results you could expand and rewrite this procedure in the methods to make it clear just what you are talking about. I would suggest even adding some graphics or tables to illustrate this procedure, since it is so important to the final depth curves and it is so difficult to describe in words alone. It is really confusing as is, and would certainly be very difficult for those not familiar with your 98 paper. (Page 6, Par 1)

> **Response:** We have rewritten the procedures in the initial methods section to correspond to the methods section in Gard (1998), and have rewritten the sections for each run to only refer to the information specific to that run, corresponding to the results section in Gard (1998). Also, we have added a figure for each run illustrating the procedure.

Comment 13: You really need to show the actual use depth data and present it with your regression (and to be able to compare depth obs for the different runs to show why the regression went to 48 ft for the fall run but only to 17-19 ft for the other runs). I do applaud your efforts to come up with a quantitative way of assessing the suitability of deep water, but when you get results like 48 ft it definitely makes one wonder about how much faith can be put in such an extreme extrapolation. In this case your extrapolation went 3.5 times beyond your deepest observation (at 14 ft). Without showing the actual data I don't know if you had "a lot" of redds in deep water or if you only had a few, and if the presence of a "few" potential outliers greatly effected the regression (which of course outliers do to regressions). I also can't really see if the fall-run redd data was truly different than the late-fall and winter-run data, and if such a difference in deep water suitability is justified. I definitely need to see the actual data to be convinced of the validity of this result. (Page 6, Par 1, last Sent)

> **Response:** The figure which we have added for each run illustrating the depth correction procedure shows the actual use depth data along with the regressions. These figures can be used to examine differences in the redd data for the different runs. We have not performed any specific tests on outliers, but based on the figure for fall-run, it does not appear that outliers had a large effect on the results of the regressions. See also response to Methods, Ed Chelak Comment 33. Also, the deepest point on any transect at the highest simulated flow was 27 feet, so the criteria were actually only used to less than twice the deepest observation.

Comment 14: It is unclear if you actually used the questionable data collected during the period of variable flows. (Page 8, Par 1, last Sent)

> **Response:** This data was used in developing criteria. A sentence has been added to the report clarifying this.

Comment 15: If you are going to discuss whether fall-run data is transferable to the late-fall run, then you need to overlay the data so people can judge for themselves if the results of the transferability tests "appear" reasonable. You need to show the actual data, show the data, show the data, show the data, show the data . . . . (Page 9-10)

Response: Figures have been added showing the late-fall-run data that we collected overlaid with the suitable and optimal ranges from the fall-run criteria.

Comment 16: (Page 10, Par 1, last Sent) Is it really legit to use the "estimated" suitable range of the fall run at 43 ft, given the level of extrapolation from the deepest observed depth? How does this value effect the outcome of the transferability test? Some discussion about the sensitivity of the test results to your estimated range of suitable habitat seems necessary here. (Page 10, Par 1, last Sent)

Response: Using the criteria value is the only way to actually test whether the fall-run criteria would transfer to late-fall. Since the transferability test failed based on the optimum/suitable test, the results would not have been changed with a different estimated range of suitable depths. Based on the overlay of the fall-run suitable and optimum ranges with our late-fall run redd data, it appears that the fall-run criteria did not transfer to the late-fall-run due to differences in the velocity distribution for the two runs.

Comment 17: Although your own site-specific late fall-run data was apparently not similar to the fall-run data (according to the transferability test), when the DFG data was added to yours and the curves produced the results look extremely similar to me. This should be discussed, probably in the "Discussion." (Page 13)

Response: See response to comment 16.

Comment 18: The discussion of annual flow variations would, in my opinion, be improved if related to a modified Figure 8 with a standard time period on the x-axis (covering the entire spawning period) with the actual sampling periods shown. (Page 12-13)

Response: The suggested changes have been made.

Comment 19: This questionable data could be identified *on your figure showing the actual data* (ie using a stacked bar graph type), and thus the readers could judge for themselves if the data appears valid for use. (Page 14, Par 1, Sent 4-5)

Response: Figures have been added showing the data for each year in stacked bar graphs. Differences from year to year likely reflect differences in the areas sampled rather than differences due to different flow regimes.

Comment 20: In this case you used the earlier measurement (which makes sense to me), but in an earlier case (fall-run, Page 5, Par 2, Sent 6) you used the mean of the two measurements. Seems like you should have a consistent rule for treating these observations. (Page 16, Par 1, Sent 2)

Response: The rule varied depending on the circumstance. For fall-run, the two measurements were made on the same day, due to two overlapping underwater video runs. In that circumstance, it made more sense to average the two measurements.

Comment 21: I am again confused due to the lack of actual data. The winter-run depth suit goes to zero at 17 ft despite seeing a redd at 15.6 ft, whereas the fall-run depth suit goes to zero at 48 ft with the deepest observation at 13.8 ft. I have to guess that the fall-run data had a lot more deep redds than the winter-run, otherwise these regression-estimated values would be bogus, but without seeing the data I am uncomfortable with these estimated values. (Page 17, Par 1, last Sent)

Response: The figures of the criteria have been modified to show the raw depth data for fall-run and winter-run. As shown in the figures added for the depth correction method, the differences between the fall-run and winter-run suitability curves is primarily due to availability dropping faster for fall-run than for winter-run.

Comment 22: You should provide a lot more information (e.g. at least the sample size and basic info comparing the two rivers) about the steelhead curves you used so people can judge for themselves if they might be appropriate. Why didn't you use your deep water adjustment method for the steelhead curve? This difference in depth curves should be addressed in the "Discussion" about the similarities/differences among your various HSC curves. (Page 19, Par 2, Sent 2)

Response: Readers are referred back to U.S. Fish and Wildlife Service (2000) for information about the steelhead criteria. I actually did use my deep water adjustment method for the steelhead curve, as discussed in U.S. Fish and Wildlife Service (2000). In that case, the analysis indicated that the ratio of use to availability increased with increasing depth, indicating that use was entirely controlled by availability. In those circumstances, Gard's (1998) method results in the depth HSC to be modified to have a suitability of 1.0 for all depths greater than the original peak of the curve.

Comment 23: The table does not give ratios for steelhead. Does the relatively high ratios used for Segs 4 and 5 mean that WUA estimates are less reliable than for Seg 6 which had lower ratios (which means that Seg 6 had a greater proportional level of sampling effort ?). If so this should be discussed. (Page 22, Par 1, Sent 2)

Response: No data was available for steelhead, and as a result we arbitrarily used the ratios for fall-run chinook salmon for steelhead. We feel that all of the estimates are sufficiently reliable, since in all cases we modeled at least 20% of the spawning habitat.

Ed Pert

Comment 1: The only shortcoming here that I see is the lack of discussion about either the representativeness of the site selection, or the goal and criteria for site selection. This is an important issue that the reader needs to judge the success of the study.

> **Response:** The ultimate test of whether PHABSIM sites are representative is whether a different flow-habitat relationship would have resulted from using different sites. The representativeness of PHABSIM sites is generally based on the number of sites. We felt that ten sites were adequate to represent the spawning habitat in the Sacramento River between Keswick Dam and Battle Creek, particularly since the sites represented at least 20% of the spawning habitat available in the Sacramento River between Keswick Dam and Battle Creek. The goal and criteria for site selection were discussed in USFWS (1999).

Comment 2: Regarding the method used to adjust use for the habitat available, I have a fair bit of experience playing with methods to adjust resource use to account for availability. Intuitively it makes sense that such adjustments are necessary given that habitat use data are often collected under a limited range of flows. Unfortunately, the only method that I have found complete comfort in is the classic work of Ivlev, which is basically a cafeteria type of experimental approach to the problem, a technique unavailable to most real-world situations. That said, we are forced to use other methods to address this issue. I have not studied your paper (could I please get a reprint?) on your method. The reason I bring this up here is that when almost any of these methods to account for "preference" are used, they are open to some level of criticism, especially when ratios are used as part of the technique. Given that this report may ultimately be scrutinized by a variety of scientists, I suggest that you add additional justification for your approach to "correct for depth criteria". I think this is especially important given that the resulting depth range extends to 48 ft. I'm not intimately familiar with habitat criteria for Chinook, so I don't know if 48 feet seems extreme (even if the HSI is extremely low at that depth). That seems awfully deep to me. If there is literature that supports that result, I suggest including it. Otherwise, this result will supply support for criticism of your correction method. I assume that you have a pretty-much canned justification for your technique that you can lift from your publication. I suggest that you add some of that justification.

> **Response:** Gard (1998) states: "The depth at which the modified HSC curve reaches zero has no biological significance; rather, it is a convenient way to describe in the criteria the rate of decline in suitability with depth." We have opted to refer to Gard (1998) to justify the depth correction method. Fall-run chinook salmon spawning has been observed in as deep as 35 feet in the Hanford Reach of the Columbia River (Chapman et al. 1983).

Comment 3: For the final WUA calculations, the reader needs to have a good understanding of the how representative the sampling was for the various river sections (or those specific areas of those sections that you were representing) to have the ability to evaluate the accuracy of the WUAs.

Alternatively, as mentioned earlier, the goal of the study site section should be made clear (I hope that I did not just miss it). I was able to get a feel for this with the hydraulic modeling document that you included (thanks, and good thinking). Based on my read of the hydraulic modeling document, it seemed to me that there were many constraints that affected the sampling locations for hydraulic modeling. Given that, as a reader unfamiliar with the specifics of the river habitat, I cannot evaluate the accuracy these WUAs. I think it would be useful to include some text regarding this issue. Of course, anything quantitative would be better than personal observations and other anecdotes. I did not see where the hydraulic modeling report addressed this issue in detail (but I could be wrong). I wonder if you have information that would address the issue of sampling representativeness. Without such language, criticism would be easy. There may be a better way to address this issue than I have outlined, but I think that the authors should give this idea some consideration that will hopefully result in additional text on this topic.

> **Response:** See response to comment 1.

Comment 4: Not necessarily representative - for clarity I think some statement(s) should be included outlining the constraints that drove site selection. (Page 2)

> **Response:** See response to comment 1. Readers are referred to USFWS (1999) for details, such as the constraints that drove site selection.

Comment 5: Was there any testing for differences in distribution among years? (Page 5)

> **Response:** While there were likely differences in distribution among years, due mainly to sampling of different areas, the combined dataset from all of the years provides the best estimate of the distribution of depths, velocities and substrates. As a result, it was not necessary to test for differences in distribution among years.

Comment 6: While I believe that you're probably correct based on your experience, it's dangerous to make these assumptions without solid justification. These are the type of things that make for easy criticism of methods, and, therefore, results. I would encourage you to justify this assumption, to the degree possible, with peer-reviewed literature or, at least, other data. (Page 6)

> **Response:** This assumption (that decreasing suitability is likely due to low availability of deeper water with suitable velocities and substrates) follows directly from the results of the method. The method separates the effects of depth versus velocity and substrate by using the same ranges of velocity and substrate for each depth interval. We disagree that a correction is not justified since the method being applied has been published in a peer-reviewed journal (Gard 1998).

Comment 7: Seems quite deep. Does the literature support depth criteria close to this? (Page 6)

Response: See response to comment 2.

Comment 8: Would make more sense to write this in non-coded language. (Page 10)

Response: The suggested change has been made.

Comment 9: Redundant - just say same as for fall-run. (Page 11)

Response: The suggested change has been made.

Comment 10: Why is the method reviewed in its entirety on next page? Not needed. Is all of this needed given the similar language on page 6? Seems very redundant. (Pages 11 and 12)

Response: We have modified this text so that only those aspects that changed from run to run, (such as suitable velocities between 0.9 and 2.82 ft/s) are given in the text for each run, with a general description of the depth correction process given in the initial portions of the methods.

Comment 11: But could turbidity or other factors have affected redd detectability? (Page 14)

Response: All data was collected when the visibility was at least five feet. At this level of visibility, we were able to cover equal areas in a given time with the underwater video cameras versus wading for shallow redds. Given that the underwater video camera was typically positioned two to three feet off of the bottom, detectability was approximately the same with the underwater video cameras as with wading.

Comment 12: Again redundant. (Page 16)

Response: See response to comment 9.

Comment 13: Again, can only the pertinent and different information be included here? (Page 16)

Response: See response to comment 10.

Wayne Lifton

Comment 1: The issue of depth alteration by spawning salmon is critical to the approach and the means of addressing the effect of that alteration. However, this is not directly addressed. It would be helpful to be more specific and to include appropriate references to the literature. The approach used by FWS to collecting these data is not new and should be cited. (Page 4)

Response: We feel that the sentence: "Data for shallow redds were collected from an area adjacent to the redd which was judged to have a similar depth and velocity as was present at the redd location prior to redd construction." directly addresses the issue of depth alteration by spawning salmon. A citation has been added to the literature.

Comment 2: The number of spawners may affect the density of redds, which may, in turn, affect the habitats used. High densities may result in the use of less optimal habitats. Some comments should be directed at the density issue and this should be put in the appropriate historical context. (Page 4)

Response: We do not feel that differences in densities would have a significant effect on HSC and thus have not added any comments on this issue.

Comment 3: The range of flows at which data were collected was in a very narrow range. This should be discussed in the context of the flow range to be modeled. Later in the report, RHABSIM was used to address availability issues during the sampling period. It also would be useful to take a similar approach to examine the range of depths and velocities that occur within the range to be simulated. This would assist the reader in understanding whether the HSC address the full range of availability that will be evaluated. (Page 5)

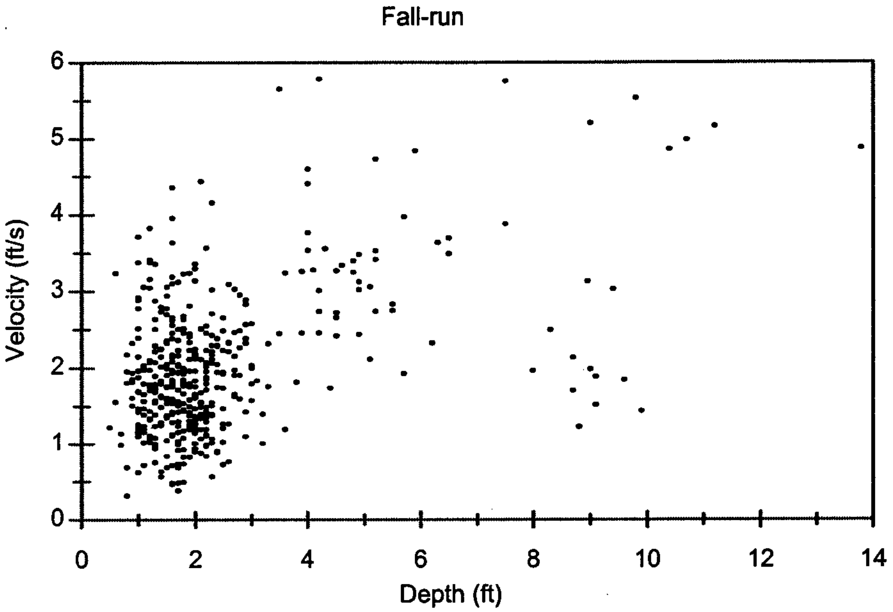
Response: Flows in the range of 4,500 to 6,100 cfs provide a sufficient range of depths and velocities, so that salmon are able to select their preferred habitat conditions. We feel that our HSC are useable for flows of 3,250 to 31,000 cfs, since HSC reflect the biological processes that drive selection of habitat characteristics, and should not change with changes in habitat availability. As a result, we disagree that the above issues need to be discussed in the report.

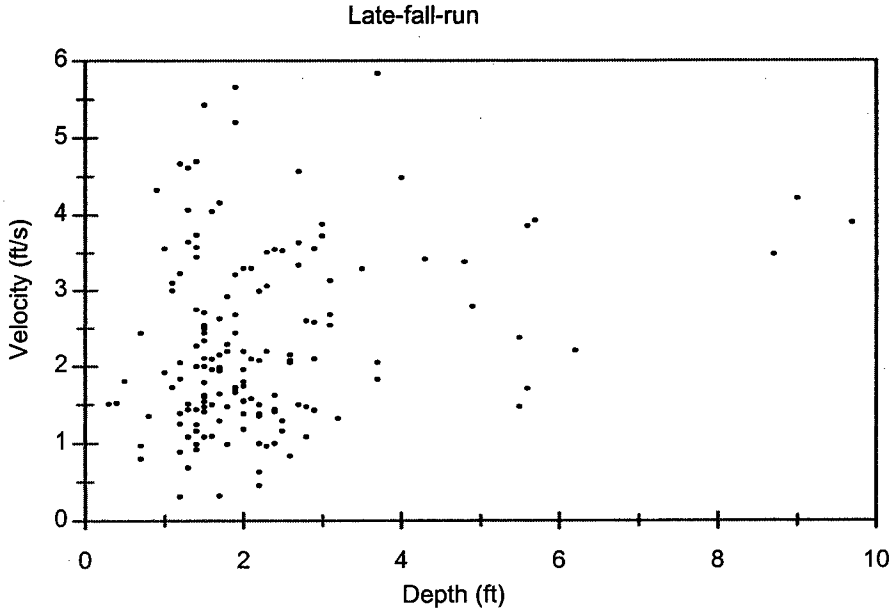
Comment 4: The stratification approach discussed was unclear. Frequently, developers of HSC attempt to use stratification to try to sample equal frequencies of availability combinations to reduce bias in utilization criteria and to reduce potential leveraging of results by availability outliers. The report should comment on the nature of the approach/strategy used. (Page 5)

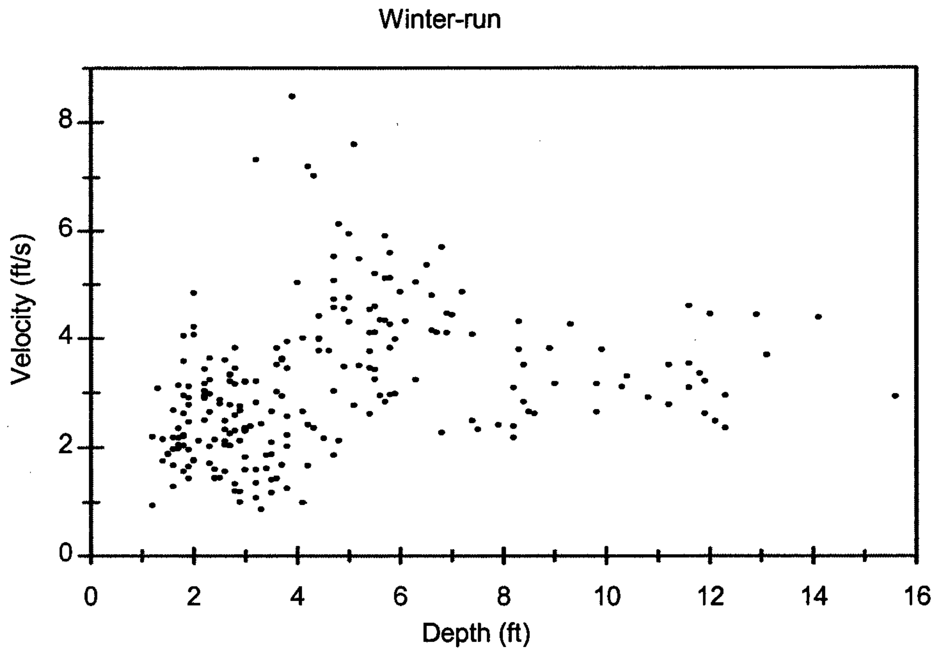
Response: The stratification approach used was to spend equal time sampling areas less than three feet deep and greater than three feet deep.

Comment 5: It would be helpful to the reader to present the typical figures used to assess utilization data. One of the figures we prepare early in the analysis is a bivariate plot of utilization to examine for covariance between depth and velocity. It is even more useful to provide a similar availability plot. Another graphics approach, that we have found useful, is to plot utilization and availability data in bivariate histograms showing velocity distributions and frequencies by depth category. (Page 5)

Response: We disagree that the above types of figures are typical or useful in this type of report. However, we have prepared bivariate plots (below) of utilization to examine for covariance between depth and velocity.







Comment 6: These comments apply to the other runs of salmon, in addition to the fall-run. The approach used to deal with the covariation and availability issues appeared to be novel. Although very similar to approaches used to deal with cover, if this approach has been used previously, it would be useful to provide a citation to the reference. The use of supporting graphics would be helpful. There are two concerns with relying on the results of PHABSIM modeling for availability information. The first is whether the modeling was performed at an appropriate scale for use with the HSC. The second is that sufficient details are represented to adequately reflect the complexity of the stream habitat for comparison with the HSC. We have found this a particular problem in small to medium streams and rivers with complex habitat. The appropriateness of the use of linear regression as a fitting approach, as used here, was not explained. Did the plotted point suggest a linear relationship? What was the r^2 of the regression? Was it statistically significant? (Page 6)

> **Response:** The reference to previous applications of this approach (Gard 1998) is cited in the report. Supporting graphics have been added to the final report. The PHABSIM modeling was performed at an adequate scale for use with the HSC, and sufficient details were represented in the PHABSIM modeling to adequately reflect the complexity of the Sacramento River spawning habitat. We feel that linear regression was appropriate, since the method was published in a peer-reviewed journal (Gard 1998). The supporting graphics can be viewed to examine the linearity of the plotted points. As noted in Gard (1998), the statistical significance of the regressions is not relevant, since the regressions are used to linearize the data, rather than test the hypothesis of the statistical significance of the relation between two variables.

Comment 7: Another question is whether bivariate HS criteria were considered? Was a more traditional approach to development of preference criteria considered? (Page 6)

Response: Bivariate HS criteria are rarely used in instream flow studies. While there was a statistically significant correlation between depth and velocity for fall-run ($r=0.47$, $p < .001$), late-fall-run ($r=0.26$, $p < .002$) and winter-run ($r= 0.30$, $p < .001$) redds, the bivariate plots above do not indicate a strong relationship between depth and velocity. Accordingly, we do not think that the use of bivariate criteria is warranted. Traditional preference criteria (Type III criteria) are no longer recommended for use (Gard 1998).

Comment 8: The use of the Thomas and Bovee approach to criteria testing is a useful approach to criteria testing. However, other approaches also are available. An approach that we have found of use is to conduct joint suitability testing using the developed continuous criteria. In the application of this type of approach, joint suitability levels for usable and optimal habitat are set *a priori.* Tests are then performed using those levels. I was pleased to note that the tests performed for fall-run chinook salmon took the approach of applying joint criteria. When dealing with criteria developed from utilization data containing covariation or interactions of the parameters, we have found this approach avoids some of the artifacts added through conversion to univariate binary criteria. In the application of the Thomas and Bovee approach using PHABSIM outputs to represent availability, the concerns expressed above with the use of PHABSIM simulations for this purpose apply, as well. (Pages 8-12)

Response: We feel that the use of the Thomas and Bovee (1993) transferability method, as opposed to alternative methods as suggested, is the best approach, since this method has been published in a peer-review journal. See response to comment 6 regarding applicability of PHABSIM simulations.

Comment 9: Were the DFG data on redd habitats collected using the same protocol as this study? (Pages 8-12)

Response: As far as we can tell from CDFG (1990), the CDFG data were collected using the same protocol as this study.

<u>**Gary Smith**</u>

Comment 1: This section is confusing in many instances. This is an important section, as it sets the stage for data collection, analyses, and conclusions. Enough information should be provided to explain what and why things were done, and to enable a reader to repeat your work. Substantial information is provided, but clarification is needed. (Page 2)

Response: Additional information on site selection, hopefully sufficient to explain what and why things were done, and to enable a reader to repeat our work, is given in USFWS (1999).

Comment 2: "...other factors." These other factors should be identified, even if they were subjective factors. (Page 2, Paragraph 1, lines 1-2).

Response: The other factors (reservoirs and channel morphometry) have been identified in the final report.

Comment 3: The title of the report indicates the report applies to the river from Keswick to Battle Creek. However, this paragraph (Study Site Selection) describes partitioning the river from Grimes to Keswick. Providing such information on the basic sections is okay, but you should then indicate (and why the study was limited) that the study is limited to the Keswick to Battle Creek sections (i.e., 4, 5, and 6), and delete all subsequent discussion of other sections (unless such discussion is relevant to development of HSC and weighted usable area/flow relationships). How many miles of river are within each section and the study area? (Page 2, Paragraph 1)

Response: This report is part of a larger study which will also address spawning in Segments 2 and 3 for fall-run chinook salmon (this will be covered in a separate report). This is indicated in the introduction. Late-fall-run and winter-run chinook salmon spawning will not be addressed in Segments 2 and 3 because Segments 4 through 6 cover, respectively, 83% and 85% of the spawning of late-fall-run and winter-run. We felt it was important for context to include Segments 2 and 3 in Table 1. Segments 1 through 3 are not addressed after that point. The miles of river in Sections 1 through 6 are, respectively, 25.5, 23.5, 52, 8.6, 18.4 and 4.5, for a total of 132.5 miles of river in the study area.

Comment 4: I suggest you be consistent in consideration of the river regarding down- or upstream references. For example, this paragraph refers to the river down to up, but in Table 1 and the report title it's up to down. (Page 2, Paragraph 1)

Response: We do not feel that it is necessary to be consistent in this regard.

Comment 5: A general description of the river and riparian zones by section is needed, and would help a reader develop a picture of the river. As drafted, readers unfamiliar with the Sacramento would have only general ideas of the river and conditions. (Page 2, Paragraph 1)

Response: We do not feel that such a description is needed.

Comment 6: "ACID" should be spelled out. (Page 2, Paragraph 1)

Response: The suggested change has been made.

Comment 7: A figure showing the study are would be helpful. (Page 2, Paragraph 1)

> **Response:** The suggested change has been made.

Comment 8: The time fame (i.e., 1989-94) should be included in the table's title. Segment 1 should be included within the table (even though it may contain no salmon spawning). The percentages for fall- and late-fall-run (93 and 98%, respectively) do not sum to 100%. Are the balances within Segment 1? Including the geographic limits (e.g., Keswick to the Anderson-Cottonwood Irrigation District facility would be helpful. It would be helpful if the table included the number of miles within each section. CDFG should be cited (footnote) as the data source. (Page 2, Table 1).

> **Response:** The time frame has been added to the table's title. We do not feel that it is appropriate to include Segment 1 in Table 1, since chinook salmon are not addressed in Segment 1. There is no chinook salmon spawning in Segment 1. The balances for fall-run and late-fall-run are found in Lake Red Bluff (between Segments 2 and 3) and below Deer Creek (between Segments 1 and 2). We feel that it would be redundant to include the geographic limits or a citation to CDFG in Table 1, since this information is given in the previous paragraph. We do not feel that it is necessary to include the number of miles within each segment in Table 1.

Comment 9: For what purpose were the sites selected? HSC work? PHABSIM? Both? You should explain and justify the validity of using the sites for each phase (i.e., HSC and PHABSIM) of the investigation. (Page 2, Paragraph 2)

> **Response:** The sites were selected only for PHABSIM. HSC data were collected in many of the sites, but were also collected in other areas. We feel that USFWS (1999) provides sufficient explanation and justification of site selection.

Comment 10: Why were two sites placed in Segment 4 and three in 5 and 6? Why not three in 4, also? Segment 6 appears to be more important for late-fall fish (based on distribution in Table 1), whereas Segment 4 appears to receive greater use by fall- and winter-run fish. Where were the sites within each segment? Were the sites close together? Widely spaced? Did the sites represent all of the spawning habitats available in the respective segments? (Page 2, Paragraph 2)

> **Response:** Only two sites were needed in Segment 4 to adequately represent the spawning habitat in that segment. The sites in Segment 4 were located at River Miles 271.7 and 279.2. The sites in Segment 5 were located at River Miles 282.7, 297.8 and 298.4. The sites in Segment 6 were located at River Miles 298.7, 299.2 and 300.6. We only had two spawning habitat types: spawning habitat and non-spawning habitat. All of the sites were located in spawning habitat.

Comment 11: Page 6, Paragraph 1, lines 14-17 indicate that HSC data were collected at "seven" sites. Presumably, this paragraph indicates eight sites were sampled. Is Page 6 a typo, was one of the eight sites deleted, or are the eight not HSC sites? (Page 2, Paragraph 2)

> **Response:** The paragraph is correct that HSC data were collected at seven of the eight PHABSIM sites. As noted in our response to comment 9, the eight PHABSIM sites were not HSC sites.

Comment 12: How were the sites selected (other than being heavily used areas)? Random? Non-random? For example, were sample sites randomly selected from a list of all heavily used areas, or were the sites subjectively selected? Did sites encompass an entire spawning area? Or, if only a portion - why? If only a portion, which portion, and why? Was the entire river width sampled at each site? (Page 2, Paragraph 2)

> **Response:** PHABSIM sites were selected from a list of areas that had the highest spawning use. USFWS (1999) gives additional information on how the sites were selected from this list. Each PHABSIM site encompassed the entire spawning area at that site. The entire river width was sampled at each PHABSIM site.

Comment 13: Sites were in areas of heaviest spawning use. Did this introduce bias into the analyses regarding habitat availability? Are/were habitat conditions in areas not heavily used the same as in heavy use areas? (Page 2, Paragraph 2)

> **Response:** We do not feel that this introduced bias into the analyses regarding habitat availability. Habitat conditions in areas that were not heavily used were likely different than heavy use areas in the amount of spawning habitat, which was reflected by the lower amount of spawning. However, we not aware of any evidence that the general shape of flow/habitat relationships (ie., peak of the curve, etc.) derived from low spawning use would be different from those derived from high spawning use.

Comment 14: Were there so many spawners that there was redd superimposition? If so, the influence on analyses should be explored. (Page 2, Paragraph 2)

> **Response:** Potential superimposition of fall-run chinook salmon redds in 1995 was noted by CDFG (1997). This would not have affected our analyses.

Comment 15: Sufficient information regarding study sites and selection should be presented to allow a reader to develop a picture of the river/sites without requiring the reader to find another report. The reader should be referred to U.S. Fish and Wildlife Service (1999) for more detailed/complete information, not for virtually all descriptive and site selection details. (This comment applies throughout the report.) (Page 2, Paragraph 2)

Response: We disagree. All of the reports are meant to be viewed together as a whole.

Comment 16: Virtually all of the above comments for Page 2, Paragraph 2 - Study Site Selection, apply to this paragraph. (Page 2, Paragraph 3 - Transect Placement)

Response: See response to comments 9 to 15.

Comment 17: Were these transects for HSC or PHABSIM activities? Were data from these transects used to generate habitat availability information used in the HSC analyses? (Page 2, Paragraph 3 - Transect Placement)

Response: These transects were for PHABSIM activities. The only data from these transects used in the HSC analyses was for the depth correction method and for the transferability test.

Comment 18: Were the transects distributed on a mesohabitat basis? Were they designed to sample only spawning habitat, or all habitats? If all habitats, the number of transects maybe/is inadequate (particularly in Segment 6 where two sites appeared to have only 1 transect and the third only two transects). The number of transects should be presented on an individual site basis, and the influence of few transects on analyses explained and evaluated. (Page 2, Paragraph 3 - Transect Placement)

Response: We have applied a biologically-based mesohabitat mapping system for spawning where there are two mesohabitat types: spawning habitat and non-spawning habitat. All transects were placed in the spawning habitat type. The number of transects in each site is presented in USFWS (1999). The number of transects for each site was selected based on the number of transects needed to represent the spawning habitat in each site. For Segment 6, the sites were uniform enough that they could be represented with one or two transects; thus, the number of transects in Segment 6 would not have had an effect on the flow-habitat relationships.

Comment 19: What were the actual flows at which water surface elevations and velocities were measured - for each site and transect? Were do these flows fall on the natural hydrograph? Are they representative of historic (i.e., unimpaired) spawning conditions (assuming the data collected were for spawning analyses)? Impaired conditions? (Page 3, Paragraph 1)

Response: Flows at which water surface elevations and velocities were measured are given in USFWS (1999). We do not feel that it is relevant where these flows fall on the natural hydrograph or if they were representative of historic or impaired conditions, since the flows are only used to develop stage-discharge relationships and Manning's n values which are used to simulate depths and velocities at all of the simulation flows.

Comment 20: Where within the water column were average water velocities measured? (Page 3, Paragraph 1)

Response: For measurements made while wading (typically less than three feet), average water column velocities were measured at 0.6 of the depth; most of these measurements were made at depths less than 2.5 feet. It was judged that, due to the sites having conditions with typical vertical-velocity curves, a measurement at 0.6 of the depth, for the few locations greater than 2.5 feet that were measured with velocity meters and wading rods, would give a sufficiently accurate estimate of the mean column velocity. For measurements made with the ADCP (typically depths greater than 3 feet), average water column velocities were computed as the average of measurements made at depth cells going down through the water column.

Comment 21: The particle sizes listed are confusing, and need clarification. For example, there are Medium (1-2 inches), Medium/Large (1-3 inches), and Large (2-3 inches) gravel categories. Is the Medium/Large category supposed to represent conditions where 1-2 and 2-3 components are equally abundant, and the "dominate" substrate components? Are they dominate/sub-dominate components? Particle size categories such as that for Small Cobble (3-4 and 3-5 inches) is another example of needed clarification. Should the Small Cobble sizes be 3-4 and 4-5 inches? (Page 3, Table 2)

Response: The Medium/Large gravel category represents conditions where the dominant size particles range in size from 1 to 3 inches; in these conditions, 1-2" and 2-3" particles were not necessarily equally abundant, but taken together were the dominant size range of particles present. We used overlapping size categories of substrate to best represent the dominant sizes of particles observed in different areas. Thus, Table 2 is correct in having small cobble sizes of 3-4 inches and 3-5 inches.

Comment 22: Define "shallow" and "deep" water here. What were the criteria? (Page 4, Paragraph 1, Line 1)

Response: Shallow is defined as areas that were wadable, while deep is defined as areas that were not wadable. Generally shallow areas had depths less than three to four feet, while deep areas had depths greater than three to four feet. We were generally able to wade areas under four feet which had a depth x velocity of less than 12 (for example 3 feet deep and four ft/s or 4 feet deep and 3 ft/s).

Comment 23: This is the first reference to mesohabitats, and that sampling was/may have been conducted on a mesohabitat basis. Mesohabitats and consideration during sample design and site selection should be described earlier. (Page 4, Paragraph 1, Line 3)

Response: The discussion of mesohabitats is only presented to show that all habitat conditions were sampled for HSC data. Since we are using a biologically-based mesohabitat typing (spawning and non-spawning mesohabitat types), geomorphically-based mesohabitat types (pool, riffle, run, glide) are not relevant to sample design and PHABSIM site selection.

Comment 24: Based on the is description, redd water depth and velocity measurements almost appear to be random. It is unfortunate that a standard approach was not employed. Was an effort made to develop relationships between data collected from different locations around the redd? How many measurements were taken beyond 6 ft from the redd? What was the greatest distance? Identify how many measurements were made by each method and distance from the redd, by sample site. (Page 4, Paragraph 1, Lines 4-8)

Response: Redd water depth and velocity measurement locations were not random, but were selected on a case-by-case basis to come closest to that present at the redd prior to redd construction. It was not possible to have a standard approach, since the location with depth and velocity most similar to that at the redd location prior to redd construction varied from redd to redd. We do not feel that it is necessary to develop relationships between data collected at different locations, since all measurements were taken at a location which was judged to have a depth and velocity most similar to that at the redd location prior to redd construction. Only three percent of the shallow measurements were taken farther than 6 feet from the redd. The greatest distance was 15 feet. Of the shallow redds measured, 86% were measured in front of the pit, 3% at a 45 degree angle upstream, 10% to the side, and only three redds were measured behind the pit. We do not feel that it is necessary to provide a breakdown of locations and distances by sample site.

Comment 25: Where within the water column were average water velocities measured? (Page 4, Paragraph 1, Lines 4-8)

Response: See response to comment 20.

Comment 26: As drafted, this sentence indicates that the Price-AA meter was equipped with a Marsh-McBirney meter. How frequently were the velocity meters calibrated/checked? (Page 4, Paragraph 1, Lines 9-11)

Response: This sentence has been changed to eliminate the indication that the Price-AA meter was equipped with a Marsh-McBirney meter. See response to methods, Ed Cheslak, comment 14 regarding calibration/check of velocity meters.

Comment 27: Generally this paragraph provides clear, concise information. This sentences could use additional information. For example, what was the water visibility? How far apart were the parallel runs/transects? How long were the runs/transects? Were the runs/transects across the stream or longitudinal? (Page 4, Paragraph 2, Lines 8-10)

Response: The water visibility was at least five feet. The parallel runs were spaced approximately 50 feet apart. The runs were made in a longitudinal direction. The above information has been added to the final report.

Comment 28: Was the grid calibration validated in the field? (Page 4, Footnote 1)

Response: The grid calibration was made in the field, and was occasionally validated by comparing the substrate size noted by eye with that observed with the video system.

Comment 29: A table showing sample period, mesohabitats sampled, fish runs sampled, flows, and redds sampled by sample site would be helpful in understanding what you did. This table should include river flow at the sample site (if possible), as well as Keswick releases. (Page 5, Paragraph 1)

Response: Graphs have been added to the report showing the sampling period for each run sampled and flows. Information is already provided in the report on the mesohabitats that were sampled. The responses to methods, Ed Cheslak, comments 16, 23 and 27 provides graphs that show the number of redds in the mesohabitat units sampled. We do not feel that it is necessary to include information split out by sample site, including flows at each sampling site.

Comment 30: Were shallow and deep redds sampled during each sample period? If so, a short statement indicating such would be helpful. If not clarification is warranted. (Page 5, Paragraph 1)

Response: Shallow and deep redds were not always sampled during each sampling period. We feel that it is only relevant that the overall time spent sampling shallow and deep redds was the same.

Comment 31: The flows in 95, 96, and 97 were virtually the same (roughly 4,500 to 5,400 cfs). River flows sampled in 99 (6,100 cfs) were a little higher. It would seem, however, in a river the size of the Sacramento these flows are virtually identical. This introduces a bias in your data and analyses. It is unfortunate 1998 data could not be used. Would it be possible to include 1998 data from actively spawning fish (rather than redds)? Could you develop sample site specific flow information so you wouldn't have to rely upon Keswick releases? The implications on the data collected, analyses, and subsequent HSC should be discussed - perhaps in a data limitations section. (Page 5, Paragraph 1)

Response: We do not agree that the range of flows introduces a bias into our data and analyses. Flows in the range of 4,500 to 6,100 cfs provide a sufficient range of depths and velocities, so that salmon are able to select their preferred habitat conditions. Flows during 1998 were generally similar to those in 95, 96, 97 and 99, and only two redds were measured in 1998. Sacramento River flows during the fall are typically in the range of 3,250 to 6,500 cfs. There is typically no more than a 1,000 cfs increase in Sacramento River flows from Keswick Dam to below Battle Creek in the fall, so flows at sampling locations would not be much greater than Keswick releases.

Comment 32: If flows and the number of redds observed at each sample site are significantly different form other sites, you need to described how the data were combined, and sample size (number of redds) bias was avoided. (Page 5, Paragraph 1)

Response: As noted in our response to comment 31, the flows between different sampling locations did not vary significantly. While there were significant variations in the number of redds at each sampling location, there wasn't any sample size bias, since all of the data was simply combined together into one dataset, with however many redds there were from each sampling site combined together in the dataset. For example, if there were two redds from one location and twenty redds from another location, the combined dataset would have a total of 22 redds.

Comment 33: Summarize how many redds were sampled by mesohabitat type by sample site. Explain how the data were combined for analyses. (Page 5, Paragraph 2)

Response: See responses to comments 29 and 32.

Comment 34: Equal effort to sample shallow and deep areas is good. However, it is possible that unequal sampling of mesohabitats (number and possibly/probably area) introduced bias into your data. For example, six bar-complex riffles, ten flat-water glides, and one side-channel riffle were sampled. Thus, without considered compilation of the data, it is possible for the data from the side-channel to be overshadowed by data from the other two mesohabitats. (Page 5, Paragraph 1)

Response: Since we are using a biologically-based habitat type system for spawning, the unequal sampling of geomorphically-based mesohabitat types would not introduce bias into our data. The presentation of geomorphically-based mesohabitat type data is only to show that all possible conditions were sampled.

Comment 35: This suggests sample sites were surveyed multiple times during a single sample period. If so, this should be described. How frequently did this occur, and within which sample units? (Page 5, Paragraph 2, Last 2 Sentences)

Response: Sample sites were not surveyed multiple times within a single sampling period. Instead, what happened in this case was that two runs ended overlapping in a small area, which resulted in one redd being measured twice (once on each run). This appeared to be the only case in which this happened.

Comment 36: I recommend considering selecting one or the other measurement (justify the selection), or deleting the data. The average data may or may not represent the conditions selected. (Page 5, Paragraph 1)

Response: We disagree. Both measurements were of the same redd; thus the average of the measurements represents the best estimate of the depth and velocity of that redd.

Comment 37: "Eliminating points" is risky. The points should be included in a figure for readers' evaluation, and you decision criteria for elimination should be stated. (Page 5, Paragraph 3, last line)

Response: See response to methods, Ed Cheslak comment 17.

Comment 38: This paragraph and methods described need expansion and clarification. It is an important paragraph as it is key to the balance of the report and HSC developed. As drafted, it is confusing, and unclear why you chose to employ this analysis. (Page 6, Paragraph 1)

Response: Readers are referred to Gard (1998) for more information about this method. It is our opinion that Gard (1998) provides sufficient clarification and basis for this method.

Comment 39: Why did you limit the analyses to 14 ft (other than you didn't see deeper redds)? It would seem that doing so would force the resultant HSC to approach "0" at 14 ft, and be "0" at any depth greater 14.0 ft. If you are attempting to develop information on availability of suitability velocities and substrates at depth, it would seem to make sense to continue the analyses beyond 14 ft. (Page 6, Paragraph 1)

Response: Fourteen feet was the deepest that there was either available habitat or redds. The selection of fourteen feet follows the methods presented in Gard (1998). In fact, the resultant HSC does not reach 0 until 48 feet.

Comment 40: This approach would force a resultant depth HSC to began to descend towards "0" as suitable velocities and/or substrate began to decrease in frequency. Hence, it assumes a link between suitable velocities/substrate and depth. While such a link may exist, it is unclear whether the method employed captures the link's characteristics. This approach appears to simply develop a picture of availability of suitable water velocities and substrates ant depth. Moreover, and more significantly, it is unclear whether the method describes water depth selection. Without additional evaluations and data, I am not convinced. (Page 6, Paragraph 1)

Response: The approach does not necessarily force a resultant depth HSC to begin to descend towards 0 as suitable velocities and/or substrate begin to decrease in frequency, because it is looking at the decline in use versus the decline in availability. If use and availability decreased at the same rate, depth suitability would remain at 1. A difference between the rate of decline of use and availability would reflect a selection of water depth. Gard (1998) provides additional evaluations and data.

Comment 41: I agree, the decline in depth HSC for depths greater than 1.75 ft is an artifact of habitat availability of suitable velocities and substrates at depth. However, it is unclear to how developing information regarding availability of suitable water velocities and substrates at depth would shed light on depth HSC at depth. Basically, your analysis (at least thus far) seems to be concluding that, sense availability of suitable velocities and substrates decrease with increasing depth, depth HSC must decrease with increasing depth. Perhaps this is true at depths in the 10-20 ft range, but I suspect it may not be so for depths up to about 10 ft. I recommend you consider re-analysis and/or another approach to considering this situation. (Page 6, Paragraph 1)

> **Response:** We have not changed the analysis since the method has been published in a peer-reviewed journal (Gard 1998).

Comment 42: Define RHABSIM. (Page 6, Paragraph 1, Line 14)

> **Response:** RHABSIM has been defined in the final report.

Comment 43: Is this part of the depth correction effort, or another evaluation? It appears to be a second evaluation. If so, it should be a separate paragraph. What is the purpose of this effort? How was habitat availability developed/determined? Is this calculating relative availability for water depth, water velocity, substrate, or all three? (Page 6, Paragraph 1, Lines 19-21)

> **Response:** This is part of the depth correction effort. The purpose of this effort is to compute relative availability and use from the availability and use information developed earlier in this paragraph. This is calculating relative availability of areas with suitable velocities and substrates for different water depths.

Comment 44: This is an interesting compilation and method to determine where depth HSC descends to "0." However, if it includes information developed via methods described earlier in the paragraph, it would seem that one would be pulling one's self up by his boot straps. It would also seem that the original character of the data could/would be lost after three regressions. (Page 6, Paragraph 1, Lines 21-31)

> **Response:** The linear regressions are simply a way of linearizing the data to capture the central tendencies of the data and avoid effects of outliers as occurs with preference methods.

Comment 45: RHABSIM results were used to determine "...available river area" with suitable velocities and substrates. The regressions appear to be based on frequencies (i.e., numbers). Were RHABSIM results used in the regressions? If so (and it appears so), clarification is required. (Page 6, Paragraph 1, Lines 21-31)

Response: The regressions were based on relative availability and use. Relative availability is calculated using RHABSIM results (habitat area), while relative use is calculated using frequencies (number of redds).

Comment 46: Were these analyses conducted on a specific flow, sample period, and sample site basis? If not, how were habitat availability and sample size bias avoided? (Page 6, Paragraph 1, Lines 21-31)

Response: These analyses were conducted using the average flow from the beginning of spawning to the end of the sampling period, and using data from the PHABSIM sites.

Comment 47: Line 22 indicated linear regression was used to remove "...noise..." from the data. Explain why it was necessary to remove the noise, and why removal of the noise does not alter the character (i.e., use, selection, and importance of specific depths to spawning fish) of the data. (Page 6, Paragraph 1, Lines 21-31)

Response: It is necessary to remove the noise to avoid problems with overcorrection of availability associated with the preference ratio methods. Removal of the noise does not alter the central tendency of the data, since linear regressions capture the central tendency of data.

Comment 48: What are the statistics of each regression. How much noise was in the data? How good is the fit? Does linear regression best describe the data, or should it be curvilinear regression? Based on distributions of habitat availability and use that I have seen, my impression is linear regression does not appropriately describe the distributions. Present the data and regression statistics (including negative regression values) in figures and tables for readers' inspection. (Page 6, Paragraph 1, Lines 21-31)

Response: Graphs have been added to the final report showing the data and regressions. As noted in Gard (1998), the statistical significance of the regressions is not relevant, since the regressions are used to linearize the data, rather than test the hypothesis of the statistical significance of the relation between two variables. Linear regressions are used because that is the method specified in the peer-reviewed journal article (Gard 1998).

Comment 49: Additional compilations are needed to justify this method. I suggest you compare use and availability of water velocity and substrate (individually and combined) with water depth use and availability to demonstrate that water velocity and/or substrate use and /or availability are not influencing water depth use and availability. Without this or similar analyses, it will be unclear whether the depth HSC truly reflect habitat selection. For example, you should demonstrate that the declines and differences in depth use and availability (lines 24-25) are real and not artifacts of the methods. (Page 6, Paragraph 1, Lines 21-31)

Response: It is our opinion that additional compilations are not needed, since this method has been published in a peer-reviewed journal (Gard 1998).

Comment 50: Although the depth curve follows from the described methods, one must ask one's self if the curve makes sense biologically. Does it make sense for depths of 1.7-1.85 ft to have 1.0 HSC values, and 1.90 ft to have a value less that 1.0? Is this an artifact of the methods? Likely. (Page 7, Figure 1)

Response: Gard (1998) presents a basis why suitability of depth could have a biological basis, apart from the effects of availability. It should be noted that the decline in suitability with depth is much less than would be found from only use data, and that the HSC value at 1.9 feet is still 0.999.

Comment 51: What are comparable HSC from other large systems? Discuss similarities/differences. (Pages 7 & 8)

Response: This is outside of the scope of this report.

Comment 52: Many of the comments regarding the fall run analyses apply to the lat-fall-run analyses. (Page 8, Late-Fall-Run)

Response: See responses to comments 11 through 51.

Comment 53: I suggest you avoid duplication wherever possible by combining methods in a methods section. (Page 8, Late-Fall-Run)

Response: We have revised the final report to combine methods where possible in an initial methods section, leaving on run-specific information in the sections for each run.

Comment 54: Describe: how many redds were shallow or deep; redds observed by flow, site, mesohabitat; how data were from different samples and sites were combined; etc. Consider and discuss potential bias introduced by unequal sampling of mesohabitats. (Page 8, Paragraph 2)

Response: Twenty of the seventy seven redds had a depth of greater than three feet. See response to comments 29, 32 and 34.

Comment 55: "...six sites..." Eight sites were referenced on Page 2. Explain. (Page 10, Paragraph 2, Line 7)

Response: HSC data were collected at six of the eight PHABSIM sites.

Comment 56: Where the differences due to depth, velocity, substrate, or combinations? (Page 11, Paragraph 1)

> **Response:** Based on the overlay of the fall-run suitable and optimum ranges with our late-fall run redd data, it appears that the fall-run criteria did not transfer to the late-fall-run due to differences in the velocity distribution for the two runs.

Comment 57: Combining 1986-88 CDFG data with your data is risky. When and where were CDFG data collected? What was the purpose of data collection. What sort of sampling design was employed? What were the flows during data collection? Which mesohabitats were sampled? Were they equivalent to yours? How were CDFG and your data combined? How were problems such as potential bias due to sample location, mesohabitats sampled, sample size differences, habitat availability, etc. address and resolved? Possibly you could examine the "transferability" of CDFG late-fall-run data with your fall run criteria? If they are not significantly different, would it be appropriate to combine CDFG data with your late-fall-run data (which is significantly different from your fall HSC)? (Page 11, Paragraph 2)

> **Response:** Given that we did not have enough late-fall-run observations to develop criteria and that the fall-run criteria did not transfer to late-fall-run, the only option we were left with was to combine our data with the 1986-88 CDFG data. The CDFG data were collected in January through March of 1986-1988 in the Sacramento River from River Mile 242.6 to 298.7. The CDFG data were collected to develop spawning HSC. The sampling design is given in CDFG (1990). The flows during data collection ranged from 3150 to 5750 cfs. It is unknown what mesohabitats the CDFG data (from 1986 to 1988) were collected in, since mesohabitat typing was not performed until 1995. From the rivermile designation for each redd, redds were found in two mesohabitats below Battle Creek; the Sacramento River has not been mesohabitat-mapped below Battle Creek. Of the remaining redds, based on the rivermile designation, it appears that, for CDFG's data that we used, the late-fall-run redds were in two Bar Complex (BC) Glides, two BC Riffles, one BC Run, two Flatwater (FW) riffles, three FW Runs, two FW Pools and one FW Glide. The distribution of CDFG's late-fall-run redds which we used in the mesohabitat units sampled is shown below. Since we are using a biologically-based habitat-mapping system for spawning, the distribution of CDFG's data across mesohabitat types has no effect on how CDFG's data fits with our data. We combined our 77 observations with the 79 observations made by CDFG which we selected (as described in the report) for a total of 156 observations which were used to develop criteria. We do not feel that there are any potential bias problems in the CDFG data from sample location, mesohabitats sampled, sample size differences or habitat availability. We are not able to conduct a transferability test of CDFG's late-fall-run data to our fall-run criteria because the CDFG dataset does not include unoccupied observations, which are required to conduct a transferability test.

Comment 58: Depth HSC manipulations: See comments for fall-run. Since the methods are the same, I suggest you present the approach in a single section, and describe variations in each their respective sections. (Page 11, Paragraph 5)

> **Response:** See responses to comments 38 to 50. We have revised the final report to present the approach in a single section, and only present run-specific differences for each run.

Comment 59: If you proceed with the methods and report as drafted, explain why there is such a substantial difference in the depth "0" value (i.e., 18.9 vs 48 ft) for late-fall and fall-run fish, respectively. (Page 12, Paragraph 1, Last Sentence)

> **Response:** Since the fall-run and late-fall-run data were collected at relatively similar flows, and thus similar availabilities, the slower rate of decline in fall-run depth suitability (versus late-fall-run) reflects that the use data for fall-run dropped off slower than the use data for late-fall-run. For example, the deepest fall-run redd had a depth of 13.8 feet, while the deepest late-fall-run redd had a depth of 9.7 feet.

Comment 60: Refer to fall-run comments. (Page 13, Figure 5)

> **Response:** See response to comment 50.

Comment 61: I understand the need to develop HSC. However, the problems associated with the data make any HSC developed incredibly questionable. I suggest you hang numerous caveats on the HSC. (Page 14, Paragraph 1, Last Sentence)

> **Response:** We feel that the report already contains sufficient caveats regarding the uncertainty in the HSC resulting from varying flows.

Comment 62: See earlier comments regarding related potential bias. (Page 14, Paragraph 2)

> **Response:** See responses to comment 13, 31, 32, 34 and 46.

Comment 63: Consideration of redds measured more than once uses different methods than used for fall-run. You should be consistent. (Page 16, Paragraph 1, Lines 1-3)

> **Response:** See response to methods, Mark Allen comment 20.

Comment 64: Discuss the differences in the decrease in depth HSC for the three runs. Fall and late-fall are similar, but winter-run's decrease occurs at a much greater depth. Reasons? Could this be an artifact of the methods? (Page 16, Paragraph 4, Line 1)

Response: The differences between the fall-run and winter-run suitability curves is primarily due to availability dropping faster for fall-run than for winter-run. See also response to comment 59. The decrease in winter-run starting at a greater depth was caused by winter-run redds generally being found in deeper water than fall-run or late-fall-run. For example, 16% and 13%, respectively, of fall-run and late-fall run redds had depths greater than 3 feet, while 61% of winter-run redds had depths of greater than 3 feet.

Comment 65: Compare and explain differences/similarities with fall and late-fall HSC, and fish from other systems. (Page 17, Paragraph 1, Last Sentence)

Response: This is outside of the scope of this report.

Comment 66: Discuss the differences/similarities in the depth "0" value (i.e., 17.0 vs 18.9 vs 48 ft) for winter-, late-fall and fall-run fish, respectively. (Page 17, Paragraph 1, Last Sentence)

Response: See responses to comments 59 and 64.

Comment 67: Refer to fall-run comments. (Page 17, Figure 9)

Response: See response to comment 50.

Comment 68: Spring-run and steelhead are not identified in the title, and have not been mentioned to this point. Perhaps the title should be general (e.g., "anadromous salmonids") and the text describes the runs/species. Or, delete spring-run and steelhead references. (Page 17, 19, Spring-Run and Steelhead)

Response: Steelhead are identified in the title. We felt that it was not appropriate to include spring-run in the title, since we did not develop flow-habitat relationships for spring-run. However, we feel that the discussion of spring-run should be included to explain why we did not develop flow-habitat relationships for spring-run.

Comment 69: It would be beneficial if you described how the depth HSC were developed, and how methods differed from yours. (Page 20, Figure 12)

Response: We feel that it is sufficient to refer readers to USFWS (2000) for a description of how the depth HSC were developed for steelhead. Steelhead depth HSC were developed using the same methods used in this report. See response to methods, Mark Allen comment 22.

Comment 70: Use of HSC data at flows substantial greater than you sampled is risky at best. For example, fall run data were collected at flows ranging from roughly 4,500 to 6,100 cfs. Apparently data from these HSC were used in simulations ranging to 31,000 cfs. Habitat availability changes as flow changes, and your HSC may or may not be applicable. I suggest you discuss this issue, and justify the flow range you expect your HSC to be usable. (Page 21, Paragraph 1)

> **Response:** While we agree that habitat availability changes as flows change, we disagree that the HSC are not applicable to flows greater than 6,100 cfs. We feel that our HSC are useable for flows of 3,250 to 31,000 cfs, since HSC reflect the biological processes that drive selection of habitat characteristics, and should not change with changes in habitat availability.

Comment 71: In addition, are your HSC usable in river reaches 2 through 6? If all of the reaches were not sampled and included in the analyses, it would appear that geographic application should be limited, or at least approached with caution.. (Page 21, Paragraph 1)

> **Response:** We feel that our HSC are usable in river reaches 2 through 6. While only reaches 4 through 6 were sampled for HSC data, the HSC would still apply to reaches 2 and 3, since HSC reflect the biological processes that drive selection of habitat characteristics, and should not change in different reaches of a given river.

RESULTS

Ed Cheslak

Comment 1: There is something wrong here. Table 4 should be a validation not a correction term. Transects (equally weighted or length weighted) ⟶ Mesohabitat ⟶ Abundance per segment ⟶ Weighting by mesohabitat $\xrightarrow[\text{map of segment}]{}$ segment totals. (Page 22)

Response: See response to Methods, Ed Cheslak Comment 3.

Mark Allen

Comment 1: The line styles need to be modified so that they are more easily distinguishable in black and white. (Figs 15-18)

Response: The suggested changes have been made.

Comment 2: This figure (Fig 17) looks very different from 15 & 16, such differences should be discussed so people will understand what is causing the differences (ditto for 18).

Response: This is outside of the scope of this report.

Gary Smith

Comment 1: Provide DWR's and your HSC in the body of the report, for comparison purposes. Describe differences/similarities. Compare with HSC from other systems. (Page 22, Paragraph 3)

Response: This is outside of the scope of this report.

Comment 2: It seems to me that your method does not correct for depth HSC for availability. Rather, it influences depth HSC based on the water velocity and substrate availability. Without additional information, this seems risky at best, and perhaps unjustified. (Page 22, Paragraph 3, Lines 5-6)

Response: We disagree. We feel that the use of this method is justified since it has been published in a peer-review journal (Gard 1998).

Comment 3: If you are correcting for depth availability (i.e., "...linearized use divided by linearized availability..." - Page 6, Line 26, and elsewhere), wouldn't your depth HSC simulate "preference?" It appears so. (Page 22, Paragraph 3, Lines 5-6)

Response: While the correction for depth availability can be characterized as preference, it differs significantly from Type III curves, where preference is calculated as the ratio of use to availability. Specifically, this method avoids the problems of Type III curves by only modifying the upper end of the depth utilization curve (Gard 1998) and by reducing the effect of outliers through the linear regressions.

Comment 4: Correcting for habitat availability as DWR did in its HSC work is considered by many to introduce problems and artifacts into HSC. This does not mean resultant HSC are incorrect. Nor, does it mean that habitat availability should not be considered in HSC development. In fact, it should. Most working in the field now try to account for habitat availability by sample design (e.g., equal area, equal effort, application limits, etc.). It appears that you attempted to account for habitat availability in you basic sampling design. However, there were matters that adversely affected consideration of habitat availability. For example, mesohabitats were not sampled equally (numbers and area); the flow ranges sampled were limited; etc. These factors do not make your HSC incorrect, but do influence HSC applicability and utility. I recommend you include a section detailing these influences. (Page 22, Paragraph 3, Lines 8-10)

Response: See responses to methods, Gary Smith comments 34, 70 and 71. Since we do not see any limitations on the applicability and utility of the HSC, a section detailing these influences is not needed.

HYDRAULIC MODELING REPORT

Dudley Reiser

Comment 1: My comments were generally limited to the first 8-10 pages of the document - I did not review the calibration details or the detailed printouts in the Appendices. As a general comment, I believe that more detail needs to be provided relative to overall methods, including site selection, transect selection, flow selection, etc..

> **Response:** It might have been helpful to add more detail to the hydraulic modeling report regarding site, transect and flow selection and other aspects of the methods, but since the report was already completed, this could not be done.

Comment 2: I realize you were interested in comments on Study Design and Technical Soundness of Methods, but I did not feel like I could really comment on those with the limited explanations provided. You will see in my comments where I specifically identified those needs.

> **Response:** Responses are given below to the specifically-identified needs.

Comment 3: Also - a couple of figures and maps depicting site locations and transect locations, etc.. is needed.

> **Response:** Figure 1 was added to the flow-habitat relationships report showing segment locations. Transect locations are shown in Appendix A of the hydraulic modeling report. It would have been helpful to add maps showing the site locations to the hydraulic modeling report, but since the report was already completed, this could not be done.

Comment 4: One other comment - I was a bit concerned about the time span over which the study was conducted - i.e. 3 years 97-99, and the influence of potential bed changes on results of hydraulic models. Given that I did not review the calibration details, I assume you must have addressed this. If not, you should make sure you discuss how you did or why that particular condition (i.e. bed elevation changes) was not a problem in this study.

> **Response:** Sites that were selected had not experienced significant channel changes (based on visual observations) in the two years we had been on the river before we established the study sites. In addition, we would have been able to detect channel changes from the water surface elevations collected in 1998 and 1999, versus those collected in 1997. Most of the water surface elevations used in the study, and all of the velocity sets, were collected in 1997.

Comment 5: Add scientific names of species. (Page 1)

> **Response:** Scientific names of species has been added.

Comment 6: State what the basis of these flows was. (Page 1)

> **Response:** USFWS (1995) states that the basis of these flows is "The Sacramento River recommendations therefore call for operations that maintain a storage pool that will enable the delivery of cool water from Shasta Reservoir through spring and summer, when winter-run chinook salmon are in the river. Within this pattern, there has been an attempt to optimize fall- and late fall-run salmon spawning flows and to reduce fluctuations that could affect spawning and rearing success during winter."

Comment 7: Provide reference for IFIM. (Page 1)

> **Response:** A reference for IFIM has been added.

Comment 8: List most relevant ones (factors used to define stream segments). Are these (segments) in up to downstream or down to upstream order? Define ACID. (Page 1)

> **Response:** This sentence has been revised to say "reservoirs and channel morphometry" instead of "other factors." Segment 1 is the farthest downstream and Segment 6 is the farthest upstream.

Comment 9: Need a Site and Reach location map. (Page 1)

> **Response:** Figure 1 has been added showing the segments.

Comment 10: What about Segment 3 and 2? (Page 1)

> **Response:** Fall-run chinook salmon and steelhead spawning in Segments 2 and 3 are being modeled in a later phase of this study.

Comment 11: Give examples of characteristics and how influence modeling e.g. slope/gradient; velocity/backwater effects and cite references of studies that have shown problem. (Page 2)

> **Response:** Characteristics which facilitate the development of reliable hydraulic models are an even water surface elevation across the transect and velocities along the transect that are perpendicular to the transect. These characteristics are assumptions of the PHABSIM model, and thus studies typically do not put transects in areas which do not have these characteristics (ie areas with transverse flow and uneven water surface elevations). Slope/gradient and velocity/backwater effects in our experience do not interfere with the development of reliable hydraulic models. Our sites had a large range of gradients and many of the sites had strong backwater effects.

Comment 12: For what (were the field reconnaissance notes reviewed for)? Site selection should be random if possible or stratified random to eliminate bias. Was any type of random selection used? If not, do sites reflect representative areas? (Page 3)

> **Response:** The field reconnaissance notes were reviewed for whether the sites had characteristics that facilitated the development of reliable hydraulic models, whether there had been any changes in bed topography, and for factors that affected the time required to conduct the study. The elements for which the field reconnaissance notes were reviewed are indentified in the second paragraph on page 3.

Comment 13: Could include (second paragraph of page 3) in a table.

> **Response:** We agree that this information could have been included in a table, but since the report was already completed, this could not be done.

Comment 14: How did you arrive at 34 (transects)? Random or representative placement? Need more explanation. (Page 3)

> **Response:** Transects were placed to represent the habitat present in the site. Upstream and downstream cell boundaries (the distance upstream and downstream that each transect represented) were determined during transect placement. Transects were placed so that the upstream cell boundary of a given transect coincided with the downstream cell boundary of the next transect going upstream, so that the transects, taken as a whole, represented the entire site.

Comment 15: What is significance of this Q (15000 cfs)? (Page 3)

> **Response:** We originally intended to simulate flows only up to 15,000 cfs, which is the highest typical release from Keswick Dam except during flood-control releases.

Comment 16: Move footnote (2) to page 1 where first mention PHABSIM. Reference IFIM. (Page 3)

> **Response:** The relevant section of the flow-habitat relationships report has the material in footnote 2 combined with the material on page 1. IFIM has been referenced (see response to Comment 7).

Comment 17: Map/figure showing transect locations. (Page 4)

> **Response:** Transect locations are shown in Appendix A.

Comment 18: Add month/year and site designations to Table 2. (Page 4)

Response: These might have made good additions to Table 2, but since the report was already completed, this could not be done.

Comment 19: Why only one transect (for Salt Creek and Lower Lake Redding sites). (Page 4)

Response: The habitat present at these sites was uniform enough that it could be represented with only one transect.

Comment 20: How many benchmarks (above ACID) is this? (Page 4)

Response: There were four benchmarks (one for each transect), plus a USGS benchmark at a USGS gage, located approximately two miles upstream of ACID.

Comment 21: Make and model of survey equipment? (Page 4)

Response: The surveying of water surface elevations and dry bed elevations was made with a Lietz/Sokkisha Model B2A Autolevel.

Comment 22: (Is) areas not wetted (the same as dry ground)? (Page 4)

Response: Yes, dry ground elevations were the portions of the transects which were not wetted at the velocity-set flow.

Comment 23: Were velocities measured at 0.6 of the depth and at 0.2/0.8 of the depth when depth > 2.5'? Explain. (Page 4)

Response: Velocities measured with velocity meters and wading rods were measured at 0.6 of the depth. Most of these measurements were made at depths less or equal to 2.5 feet, since the ADCP was used for areas deeper than three feet. It was judged that, due to the sites having conditions with typical vertical-velocity curves, a measurement at 0.6 of the depth, for the few locations greater than 2.5 feet that were measured with velocity meters and wading rods, would give a sufficiently accurate estimate of the mean column velocity.

Comment 24: Two years? Test for bed elevation changes. (Page 4)

Response: See response to Comment 4.

Comment 25: Make and model (of ADCP and laser range finder)? (Page 5)

Response: The ADCP is a 600 kilohertz Broadband model manufactured by RD Instruments. The laser rangefinder was an Advantage model manufactured by Laser Atlanta.

Comment 26: What were (measured intervals)? Not clear as to why this was done. (Page 5)

Response: The measured intervals refers to the spacing between verticals. The verticals were spaced to capture changes in depth, velocity and substrate going across the transect. The verticals were typically spaced five to ten feet apart.

Comment 27: A figure depicting this process would be useful (Page 5)

Response: We agree that a figure depicting this process might have been useful, but since the hydraulic modeling report was already completed, this could not be done.

Comment 28: How was boat kept perpendicular to Q; siting across transect? (Page 5)

Response: The boat driver visually kept the boat perpendicular to the flow using the buoy on each side as a visual guide.

Comment 29: Figure or photo of camera? (Page 5)

Response: The camera assembly is a minor modification of the carrier in Groves and Garcia (1998). Groves and Garcia (1998) includes a figure of their carrier.

Comment 30: Reference (for substrate descriptors and codes). (Page 6)

Response: The substrate descriptors and codes are our own system, but are a modification of Brusven's (1977) index.

Comment 31: Why not just say - substrates were visually assessed within the same transect segments that were wadable; remotely assessed in deeper areas. (Page 6)

Response: We agree that such a change in the text would have been better, but since the hydraulic modeling report was already completed, this could not be done.

Comment 32: Describe QA/QC process. (Page 7)

Response: The main QA/QC process for compilation and checking of data was to examine bed and velocity profile plots for anomalies.

Comment 33: Why RHABSIM if USFWS? I would have used PHABSIM since this was USFWS study. Also, need to describe what RHABSIM is - Maker? (Page 7)

> **Response:** RHABSIM was used because (at the time) PHABSIM had a limit of 100 verticals per transect. Many of our transects had more than 100 verticals. RHABSIM is made by Thomas R. Payne and Associates.

Comment 34: I assume you have a complete QA/QC and calibration report for <u>each</u> site. These should be included in the final report. (Page 7)

> **Response:** The QA/QC and calibration information for each transect is given in Appendices B and C.

Comment 35: Were these the flows for which complete field data were collected? Need more descriptive title. What is Bend err? (Page 8)

> **Response:** These were the flows at which a WSEL, velocity set and/or discharge was collected. All three data types were not collected at all of these flows. A more descriptive title might have been helpful, but since the hydraulic modeling report was already completed, this could not be done. Bend err is the percent difference between the discharge at Bend Bridge, calculated from Keswick releases and tributary flows, and the measured discharge at Bend Bridge - see footnote 7.

Comment 36: These types of notes best in Appendix. (Footnote 8)

> **Response:** This might have been a good change, but since the hydraulic modeling report was already completed, this could not be done.

Comment 37: Better title (for Table 5). (Percent difference) of what? (Page 10)

> **Response:** A better title might have been good, but since the hydraulic modeling report was already completed, this could not be done. As noted in Footnote 7, the percent difference was the percentage difference between the gage-based flow in Table 4 and the measured flow in Table 5. For example, for Salt Creek, 3% = 100 x (14600 - 14228)/14600.

Comment 38: This table (Table 6) needs better explanation (Page 12)

> **Response:** A better explanation might have been good, but since the hydraulic modeling report was already completed, this could not be done.

Comment 39: I assume you will have better photos in report. Need to have an index map that shows transect relative to study segments. (Page 19)

> **Response:** The original report had better photos - details were lost during photocopying. An index map was added to the report, ""Flow-habitat relationships for steelhead and fall, late-fall and winter-run chinook salmon in the Sacramento River between Keswick Dam and Battle Creek," showing the location of the study segments. It would have been good to have an index map showing the locations of the transects in each segment, but since the hydraulic modeling report was already completed, this could not be done.

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